7C *3.11

R39762

Digitized by the Internet Archive
in 2015

https://archive.org/details/b21729876

THE EVOLUTION

OF

MORBID GERMS:

A CONTRIBUTION TO

TRANSCENDENTAL PATHOLOGY,

BY

KENNETH W. MILLICAN, B.A. CANTAB., M.R.C.S.

Fellow of the Medical Society of London.

ROYAL COLLEGE OF PHYSICIANS, EDINR.
LIBRARY

LONDON
H. K. LEWIS, 136, GOWER STREET, W.C.
1883.

TO

MY FRIENDS AND TEACHERS,

W. H. BROADBENT, ESQUIRE, M.D., F.R.C.P.,

AND

EDMUND OWEN, ESQUIRE, M.B., F.R.C.S.,

THESE PAGES

ARE GRATEFULLY INSCRIBED.

PREFACE.

WITH a view to obviate the necessity of a Preface, I had prefixed some words of apology to my introductory chapter. One or two points have, however, occcurred since the volume went to press that call for a few words of comment.

In the first place, much exception has been taken to the use of the word "Hybrid," as applied to disease. The choice of the word was unfortunate, inasmuch as it has induced much misapprehension of my second Circular question. I did not intend to imply, as has been inferred by some, a mongrel germ, the result of conjugation between different species, but merely certain vague and indefinite clinical phenomena, which would appear to be a combination of those proper to two or more diseases. The term "Nondescript" more aptly expresses my meaning, and that term I have substituted for the word "Hybrid" in the present work.

Also with regard to the term "Autogenetic." Many correspondents have read the last clause of my quotation in the Circular, as asserting Rötheln to be autogenetic (*i.e.*, of common kindred) with Scarlatina and Measles. The true reading is that I consider Rötheln to be undoubtedly autogenetic, *i.e.*, self-originating or capable of *de novo* origin. In this opinion I am confirmed by German friends, in which country, of course, Rötheln is most at home.

I would here point out that a recent view as to the relation of Bacteria and disease, viz.: that their site is a matter of importance, is as compatible with the doctrine herein advanced as any other yet promulgated. MM. CORNIL and BABES made a communication in the early part of this year to the *Société Médicale des Hôpitaux*, in which they point out a distinct relation between certain specific microbes and particular histological seats, *e.g.*, the Erysipelas cocci occupy the lymph spaces, while those of Small Pox chiefly inhabit the vacuolated cells of the *rete mucosum* (*Lancet*, July 25th, 1883). Now the struggle for existence and the search for the requisite pabulum may easily, after acting upon many successive generations, originate an hereditary power of selection which would account for the constancy of the localisation.

A further matter to which I cannot avoid referring, although it is fully dealt with in the text, is the question of Generations *versus* Time in the history of the process of development. It is to me extraordinary how many appear to have supposed that the process of development to which I refer takes place in the life history of a *single individual of the species*. I quote a statement from a reply to my Circular which illustrates this fact :—"Until an omni-germinating egg has been discovered that will hatch out—say a turkey under a turkey, or a sparrow or an eagle under either of these maternal incubators, I should not be inclined to believe that specific infection will develop any other specific disease than its own." Now, the author of this criticism quite evidently understands me to propose that a certain Scarlet Fever germ A may in the course of its individual lifetime be developed into a Diphtheria germ. I need scarcely tell any evolutionist I mean no such absurdity. But I do believe that, given a change of environment acting successively upon a certain Scarlet Fever germ A and its offspring, for, say 1,000 generations, may succeed by the end of that period—in Bacteria representing about a fortnight—in impressing a very marked change of character upon the race. Such a change, in fact, in the selection of pabulum, site of localisation, mode of life, excreta, and consequently in the manner in which the physiological forces and functions of their host re-act to their presence, as to succeed in ultimately transmitting Diphtheria in place of Scarlet Fever. Of course, the probability of this will be in the inverse proportion to the specific stability acquired by the germs of any particular disease. At the risk of being deemed very tautological, I must insist upon the fact that the perceptible workings of the law of evolution are a matter of **successive generations**; and that the **time occupied will vary with the duration of individual life,** and the rapidity of reproduction.

As to originality, I do not, as has been implied, claim originality for the suggestion to apply the Darwinian doctrine to disease; but for the particular method in which I have carried out that suggestion. I have been referred, not with an excess of courtesy, in a correspondence in the *Medical Times and Gazette* of Oct., 1883, to pages 605 and 606 of the English translation of the Eighth Edition of NIEMEYER'S *Text Book of Medicine* (Chapter on *Febris recurrens*), as entirely forestalling me in the views now advocated. I quote the passage referred to:—

"I believe generally that the history of epidemics strongly supports the correctness of DARWIN'S theory of the origin of new species. For my part, I have no doubt that, in the course of centuries, new infectious diseases have developed and taken the place of others that formerly prevailed. In the writings of the Ancients we find wonderfully accurate descriptions, even of forms of disease, whose recognition and distinction offered the greatest difficulties. There can be no doubt that the regular and easily recognised combination of symptoms characterising Measles, Scarlatina, Typhoid Fever, &c., would not have escaped the sharp observation of HIPPOCRATES, if it *(febris recurrens?)* had existed then as it does now about the native plan of the great ASKELPIAD.

If the correctness of this hypothesis be accepted, and the dependence of contagious diseases on an infection of the body from low organisms be regarded as certain, the appearance of new infectious diseases must seem a proof of the development of new species of organic beings. Should not the varied malignity shewn by epidemics of the same disease at different times, and certain peculiarities of the individual epidemics, be most simply referred to *slight* modifications, and the gradual passage of certain phases of infectious disease into other forms as *extensive* modifications of the low organisms?"

That NIEMEYER here suggests that the Darwinian doctrine might be applicable to the original development of the specific infectious diseases, there can be no doubt. That he does not attempt to apply the doctrine is to my mind equally clear.

But from the Evolutionist's point of view, even this passage appears strange when considered in the light of the following contradictory statements from the same edition :—

"I do not agree with most recent authors in considering the question settled in regard to whether Exanthematic Typhus be exclusively propagated by contagion, or if it be also due to miasm. There is no solid ground for such assertions as this, 'that a disease is either *only* miasmatic or *only* contagious,' and even the persons making them do not always strictly adhere to them."—Page 565.

Compare with this the two following :—

"Measles is a purely contagious disease. There is no doubt that a person is *never affected with Measles without having been infected by a person with measles.*"—Page 523.

"Relapsing fever is among the acute infections about whose propa gation by contagion there is no doubt, but we hesitate to proclaim this as a purely contagious disease, such as the acute Exanthemata, Measles, Scarlatina, and Small Pox, which *never occur in a person who has not been infected by a Measles, Scarlatina, or Small Pox patient.*"—Page 604.

I fail to see any attempt to apply the doctrine of Evolution to the present development of the specific diseases here.

It is, however, fortunately, quite immaterial whether these views are original or not. It is certain they are not commonly accepted, and the main question is, therefore, whether they are correct. To that question nothing but careful investigation and patient observation can supply an answer; and I commend their consideration to the Profession all the more confidently, that they are deemed worthy of it by Sir JAMES PAGET, Dr. BROADBENT, and many others, to whom I here tender my grateful thanks for words of kindly encouragement, in this endeavour to open up the subject.

K. W. M.

Kineton,
Warwick,
November, 1883.

CONTENTS AND SYNOPSIS.

PAGES

Preface.

Chapter I.—Introductory .. .. 3 to 9

Case of apparently concurrent Typhoid, Diphtheria, Typhus, and Small Pox, of *de novo* origin, 4. Report of the Medical Officer of Health, 7.

Chapter II.—Specific Diseases and the Germ Theory 10 to 20

Zymotic Diseases, their common characters, 10. Principal views as to nature of the poison, 11. The Germ Theory in Surgery, 12. Arguments on behalf of Germ Theory: (i.) Analogy to fermentation, 13; (ii.) Organisms found in the body, 14; (iii.) Reproduction of poison, 15; (iv.) Periods of incubation, 15. Arguments against (i.) Clinical, 16; (ii.) Pathological, 17; (iii.) General, 18. Clinical arguments (cases of *de novo* origin, and anomalies of origin and character), the most forcible, 19.

Chapter III.—Anomalous Cases & the Germ Theory 21 to 43

Three types: (i.) "Variability in transmission;" (ii.) "Diversity of character from unity of origin;" (iii.) "Nondescript" diseases. To these add "Progressive Development," 21. The Anomalies are numerous, 22. Hughlings Jackson on Clinical methods of observation, 24: (i.) Variability in transmission—Dr. Meredith's case, 25; Dr. Holland's cases, 26; Dr. Parsons' cases, 27; Surgical Scarlatina, 28; Puerperal Scarlatina, 29. (ii.) Diversity

CHAPTER III.—*Continued.*

from unity, 29: Dr. CHURCHILL on Epidemic types, 30; Dr. DICKINSON on Croup and Diphtheria, 30; Dr. DUKES on Diphtheria and Typhoid—two cases, 30; Dr. MEREDITH on Diphtheria and Mumps, 32; Dr. KING CHAMBERS on "Typh.-Fever," 32; Dr. HOLLAND on Diphtheria and Scarlatina, 35. (iii.) Nondescripts, 35. Dr. GREENHOW on Diphtheria, 36. Dr. WILSON on Measles at Cheltenham, 36. Dr. BOND on Scarlatina, Measles, and Catarrh, 37. Dr. BEEVOR on Measles Rash in Typhoid, 38. The orthodox doctrine of the Germ Theory stated, 38. Professor HUETER'S views on Specificity, 39. Professor STOKES on the same, 40. Dr. PLAYFAIR on Specificity and the Germ Theory, 41. Dr. KING on *Contagium vivum*, 41. An explanation of difficulties offered by Darwinism, 43.

CHAPTER IV.—APPLICATION OF EVOLUTION TO MORBID GERMS .. .. 44 to 73

Evolution in Disease: Dr. W. H. PEARSE on, 44; Dr. W. J. COLLINS on, 45. *De novo* origin of disease compatible with Germ Theory independently of heterogenesis, 46. Acquisition of specific properties by germs, 46. 'Specific' germs the result of 'specific' views on Clinical features of disease, 47. Dr. CHAMBERS on the 'person apt to be poisoned,' 48. Sir JAMES PAGET on the grain and the soil, 48. M. BOUCHARDAT on the same, 49. Dr. SANSOM on the same, 49. Clinical instances, Measles in Fiji, in the Faroë Islands, Small Pox among the Micmac Indians (Footnote), 50. The process of development, 51. Effect of Bacterium on Medium, 51. Adjustment to environment,

Chapter IV.—*Continued.*

52. Dr. Thorne's views on this subject (Footnote), 52. Relation of organism to disease; Cause of Clinical Phenomena: (i.) abstraction of material, (ii.) addition of contaminating excreta, 53. Arguments in support of (i.) 53. Both causes probably important, 55. Pasteur's experiment with Fowl Cholera, 55. Ziegler on the effect of Bacteria upon their nutrient medium, 56. Dr. Wilson on variation in disease germs, 57. The "correlation of diseases," 57. Fraulein von Chauvin's experiment with the axolotl, 58. Simon on the Evolution of Contagia (Footnote), 59. Buchanan on the same, 59. Buchner's experimente with *B. fœni* and *B. anthracis*, 60. Virchow's opinion thereon, 60. Naegeli's experiments on variation of ferments, 61. The variation of *Torula* (Footnote), 61. Dr. Klein on a morphological variety of *B. anthracis*, 62. Dr. George Harley's experiments on effect of medium upon germs, 62. His conclusions, 63. The course of evolution, 64. Dr. Creighton on the physiological analogies of the specific infective diseases—an explanation thereof, 67. Ziegler on the struggle between tissue cells and bacteria, 68. The explanation of exhausted susceptibility, 69. This process of evolution active at the present day, 70. Time not the standard of evolution, but generations, 71. Dr. Roberts thereon, 72. Darwin thereon (Footnote), 72.

Chapter V.—The Process of Evolution in Disease 74 to 92

Sir James Paget in Bradshawe Lecture, 74. Mr. Lawson Tait thereon, 75. Dr. Chatterjee on Typhoid in India, 76. Dr. Creigh-

CHAPTER V.—*Continued.*

TON on Yellow Fever, 76; and on Dysentery and Typhus, 77. Dr. THORNE THORNE on Croup and Diphtheria, 79. Mr. EDMUND OWEN thereon (Footnote), 80. My own case of Evolution of Scarlet Fever, 81. Objections to the theory considered, 85. Instances of bias from ZIEGLER, 86. The cycle of Evolution in microorganisms, 89. This view explains three important points (Footnote), 91. General explanation, 91.

APPENDIX .. .. 93 to 106

Copy of Circular, 93. Names of Gentlemen who replied, 95. Analysis of answers, 97. Tabulated list of cases indicating (i.) Variability in Transmission, 98; (ii.) Nondescripts, 100; (iii.) Diversity from Unity, 103. Dr. COLLINS' Letter on Specificity and Evolution in Disease, 106.

ERRATA.

PREFACE—Page 4, line 2 from top—for 'plan' read 'plain,' for 'ASKELPIAD' 'ASKLEPIAD.'

PAGE 84, line 6 from bottom—for '*à priori*' read 'antecedent.' Line 4 from bottom—for '*à priori* evidence' read 'probability.'

THE

EVOLUTION OF MORBID GERMS.

CHAPTER I.—INTRODUCTORY.

THE importance of, and difficulties involved in, the consideration of a subject so abstruse as that with which I am about to deal, render it necessary for me to speak with great diffidence. Indeed, in the face of the acknowledged genius and experience of the men who have hitherto contended for and against the "Germ Theory of Disease," and that without arriving at conspicuous unanimity, any other frame of mind would indicate extreme presumption.

It is, however, that very antagonism of authorities of repute, greatly aided by the kindly encouragement and attention of many well qualified to judge on the merits of the question, that emboldens me to publish the following considerations on the subject; for they arose out of an endeavour to arrive at a theory which, while admitting all that is positive on either side of the controversy, should explain and be in harmony with those exceptional cases of disease whose existence is the chief support of conflicting views.

The substance of much that follows was communicated by me to the Medical Society of London, in February, 1882, in a paper entitled "Some suggestions for a modification of the Germ Theory of Disease." So far as I was then aware the views I enunciated were absolutely new; but subsequent research has shewn me that many of the main points have been advanced at various times and places, by other writers or speakers, and that these *disjecta membra* when articulated, so to speak, form a complete whole with a marked resemblance to the views I then enunciated.

With these preliminary observations I will at once proceed to relate a remarkable case which forms the basis of all my considerations. Indeed, it was the unusual difficulty involved in the explanation of this case that first drew my attention to the subject, and initiated the train of thought of which the present work is the outcome.

I first saw my patient, a strong healthy farm labourer, 20 years of age, on the third day of a severe feverish attack, when I found the evening Temperature, 102° F. He had been 'out of sorts' some week or ten days previously. He had Diarrhœa, the stools being distinctly of a **'pea soup'** character, and there were **tenderness and gurgling in the ilio-cæcal fossa.** He lay prostrate, at times delirious, with dry brown parched tongue, sordes about the teeth and lips, and two or three **diphtheritic looking patches** on the tonsils. There was considerable pain in the head, with intolerance of light and sound.

I succeeded with difficulty in detaching a small portion of the membrane from the tonsils, and left the subjacent surface raw and bleeding; but found on my next visit that the patch had been renewed. The patient was ordered diffusible stimulants and a carbolic wash for the throat.

On the twelfth day of the fever a crop of **rose spots** were visible on the abdomen, and around these, ink circles were drawn. The chest also displayed a peculiar "marbled" appearance, livid in hue, being more blotchy and indistinct over the pectoral and infraclavicular regions, and becoming more discrete and "mulberry" looking over the sternun, thus **resembling the rash of Typhus both in character and distribution.** The Typhoid state was very marked and the Temperature had risen to 105° F. The Pulse was 140 and the Respirations 34. Altogether the aspect of the case looked very hopeless, and the high Temperature was remarkable.

On the fifteenth day, three days after this extreme Temperature, it fell again to 100° F., and the patient seemed somewhat easier. The mother called my attention to some **"shotty" pimples** on the buttocks. There were also a few on the wrists, but these were less marked.

The seventeenth day, two days later, saw the first crop of rose spots almost entirely replaced by new ones. The purple ones were fading. The bases of the pimples on the buttocks and wrists were inflamed. By the twenty-first day of the fever, or six days after the appearance of the papules, these latter had become **umbilicated vesicles.** The mother remarked that "he looked for all the world as if he had been vaccinated;" while the father thought the eruption "like the cowpox."

On the twenty-fourth day, the patient's brother informed me that he had become feverish and excited, and on visiting him the following day, I found the **vesicles had suppurated.** The Temperature was then 105·4° F.

The thirtieth day saw the pustules scabbed over and the patient much better; the Diarrhœa had entirely ceased, and the ilio-cæcal tenderness, &c., had disappeared. From this time he began to mend rapidly.

Now it will be readily admitted that this case presents many features of interest. (1) The **combination of symptoms** was remarkable. But for the astounding nature of the assertion one would have felt inclined to say that the patient was suffering from the concurrence of Enteric and Typhus Fevers with Diphtheria and Small Pox.

(2) It was found quite **impossible after the most careful search to trace any source of contagion for any one of the four diseases** which were here presented

or counterfeited. There was no possible chance of ordinary infection, for

(a) The patient lived in an **isolated village** from which he had not been so far as four miles for many months.

(b) Neither he, nor anyone in the family, so far as could be discovered, had **held any communication** with any person labouring under any kind of infectious disease.

(c) There were **no cases of infectious disease of any kind** existent at the time in the village or immediate neighbourhood.

Dr. George Wilson, the Medical Officer of Health for the Mid-Warwickshire Sanitary District, came down at once on the first report of Typhoid Fever to investigate the origin of the outbreak. The following is an extract from a letter to me in answer to some enquiries I subsequently addressed to him relative to the case. He says—"I well remember B's case and the difficulties I had of arriving at any reliable conclusion as to the cause of his illness. In the absence of any other probable cause, it appeared to me possible that B, who was working on the roads, and who as his father assured me, was in the habit of drinking water out of any way side pools to slake his thirst, might have contracted the illness in that way. But as other cases cropped up in the same row of houses, and in a house adjoining the row, the outbreak pointed rather to defects of a local nature, and of these there were two which could not fail to have a considerable influence. First, there was the nuisance arising from the deposit of excremental matters from the closet pails at the bottom of the gardens and in front of the houses; and secondly, it was discovered that there was an old cesspool near the end of the row, into which the drains conveying the slops emptied, and which formerly received the drainage from the privies before these were converted into pail closets. This cesspool had not been cleaned out for an unknown period, and the effluvia from it escaped through the impartially trapped gratings in front of the houses. No doubt when the disease was originated, it spread to a certain extent by excremental infection. I need hardly say that I carefully considered the possibility of water pollution and of milk agency, but there was no evidence, so far as I could ascertain, to support either of these views."

With reference to this report I would here remark that Dr. WILSON came down at the commencement of the case, when it was supposed to be simply "Diphtheritic Typhoid," and was consequently unaware of the extraordinary later developments.

(3) **No form of disease was transmitted** from this patient to any inmates of the house. Other cases of Typhoid and Typhoidal Diarrhœa followed in the village, and two

cases of **"Scarlet Fever"** occurred while my patient was convalescent, in the house opposite and close to, for which the medical attendant informs me no origin was ever traced.

The problem thus presented was to **explain the apparent *de novo* origin of a disease resembling in some marked characteristics four commonly supposed distinct "specific" diseases,** two of which at least are generally thought to be always necessarily derived from pre-existent cases.

To myself, a firm believer in the Germ Theory of Disease, this problem was essentially a startling one, and I began to review the ground to see whether it could stand in the face of facts such as these (which enquiry soon convinced me are much more numerous than is commonly supposed), or whether some explanation could not be found which should reconcile these seeming contradictions.

CHAPTER II.

BEFORE plunging *in medias res* in regard to the subject of our enquiry, it may be well to give some general consideration to the class of diseases known as the Zymotic or acute specific infectious diseases. The individual members of this class are not all equal in their character of 'specificity;' Small Pox undoubtedly possessing it in a high degree, while Diphtheria and Typhoid are so variable as to be regarded by many as non-specific.

Among those that occur in this country may be mentioned Erysipelas, Rötheln, Diphtheria, Typhoid, Typhus, Measles, Scarlatina, Whooping Cough, and Small Pox. To these would be added by some, Puerperal Fever.

These diseases have certain points in common. (1) They are usually **originated by contagion** in some form or another. (2) They have all a period more or less marked and regular of **incubation**—*i.e.*, a period during which, so far as outward manifestations are concerned, the poison appears to be dormant or inactive. (3) They are usually accompanied with **constitutional disturbance manifested by increased Temperature and derangement of the Circulatory, Excretory, and Respiratory**

systems. (4) They tend to run a **definite course** which is, speaking broadly, more or less constant and identical in the same disease. (On these points I shall have more to say presently; I am now merely recapitulating commonly accepted views). (5) They are usually accompanied by **characteristic lesions.** (6) They can be **conveyed from the infected person** either directly or mediately to those who come into contact with him. (7) And finally, in many cases at least, one attack appears to **exhaust the susceptibility of the patient** to the disease, and to render him incapable of contracting it in the future.

That these diseases are due to some kind of poison which is transferred during the act of contagion, was naturally imagined at an early period in their history; and speculation exhausted itself in every direction to discover the nature of the poison. The principal views which have obtained in regard thereto are—"(1) That it is some *subtle entity* which it is impossible to detect. (2) That it is a chemical substance, which, as knowledge improves may be isolated, and which may be solid, liquid, or as is most commonly supposed, in the form of a *volatile* gas. (3) That it is some 'albuminoid matter in a state of rapid chemical change,' which gives rise to a fermentative or *zymotic* action in the blood and tissues (hence the name *zyme.*) (4) The

Germ theory is that which is most commonly entertained in the present day, which supposes that every contagious disease is due to *living germs*, specifically distinct from all others." (Robert's Handbook of the Theory and Practice of Medicine).

This latter view is by no means new. It is merely the revival with more point and elaboration, of a doctrine which dates back far into the early history of Medicine.

But although in recent times speculation had for some years directed itself towards this field of thought, the first important practical application of it may be safely claimed for the system of antiseptic surgery now inseparably linked to the name of Lister. It is in the region of surgery rather than in that of medicine, that visible and tangible results of therapeutic methods may be expected; and the brilliant results of "antiseptic surgery" soon cleared the way for a general acceptation of the "Germ Theory" as applied to disease. It is now, perhaps, scarcely too much to claim that this doctrine numbers among its adherents by far the larger proportion of the Medical Profession; and the brilliant and invaluable researches of KOCH, KLEBS, KLEIN, PASTEUR, GEORGE HARLEY, GREENFIELD, BUCHNER, BURDON SANDERSON, BRAIDWOOD and VACHER, with many others, are now matters of ordinary medical teaching.

Without entering into details respecting the other theories above referred to, we may briefly recapitulate the principal arguments for and against this important doctrine. It may be briefly stated in behalf of the "Germ Theory" that—

I. The discovery of the *Torula Cerevisiæ* as the principal agent in the production of the process of alcoholic fermentation, and of other **organisms occupying a like place in regard to other kinds of fermentation,** renders it antecedently probable that phenomena so similar to these processes, as those displayed by the acute specific diseases, should have their origin in the workings of a similar organism.

The parallel is most clearly drawn out by Dr. JOHN SHEA in an article in the *Medical Times and Gazette* of December 30th, 1879, of which the following is an abstract :—

"It is by no means difficult to draw an analogy between the process of brewing and the process that goes on in the human body affected by zymotic disease . . . .
There is—

"1. An organic fluid, the blood, susceptible of change.

"2. The fluid may be fermented by the introduction of disease germs which act as ferments ;

"3. The fermentive process causes a rise of Temperature; and

"4. The kind of fermentation will depend on the kind of ferment or germ introduced, just as the quality of the yeast will govern the fermentation set up in the Brewery.

"5. The act of fermentation being accomplished, the mature form of the ferment disappears or perishes, but in so doing gives rise to a fresh crop of spores, which *after an interval* redevelop the mature form of ferment, and with it the fermentative process, and a consequent rise of Temperature;

"6. This sequence can be checked by the application of cold or by the administration of some bitter such as quinine, which acts as an antiferment just as the bitter of the hop acted on the beer."

Moreover, not only is the alcoholic fermentation associated with a microorganism, but the lactic, butyric and other fermentations also owe their rise to organic interference. A stronger analogy with the process of fermentation is to be found in the fact demonstrated by PASTEUR that **if the organism of Fowl Cholera be cultivated out of the body in Chicken broth, it produces therein a fermentative process resulting in the formation of an organic alkaloid.**

II. The evidence connecting the **occurrence of organic forms in the blood, tissues and secretions** of those affected by "specific" diseases with the causation thereof, is rapidly accumulating and becoming strengthened. To the microscopical observation of these organisms in the blood, &c., has been added their culture in various nutrient fluids out of the body, and experiments proving that the injection of the organisms from the culture fluid into other animals will redevelop in the latter the symptoms of the disease. This has been abundantly shewn in the case of the *Bacillus Anthracis*, and also with the *Bacillus Malariæ* and others. Microorganisms have now been more or less definitely connected with Anthrax, Relapsing Fever, Elephantiasis, Tuberculosis, Typhoid Fever, Diphtheria, Measles, Scarlet Fever, Syphilis and other diseases.

III. One of the most important evidences is the extensive and almost unlimited **reproduction of the poison** in the system ; and finally—

IV. The existence of **'periods of incubation'** is thus explained, since they would, as in the analogy of brewing drawn above, represent the period of quiescence occupied in the preliminary multiplication of the organism.*

---

* It may not be amiss here to refer—not by way of argument, scarcely even by way of illustration, but more as an interesting fact,—to an observation of Dr. WALTER MOXON, who some years ago was watching floscularia in his aquarium with a view to studying their nerve organs, when these organisms began to fade and die away before he had time to finish his observations. He remarks, "As the creatures sickened, they became turbid instead of clear; and whilst I was endeavouring to follow the nerves from the ganglia, I saw that the obscurity which clouded them was due to the presence of countless bacteria, exactly such as I am now familiar with in KOCH's figures. I now know that I was witnessing an epidemic of bacterium fever in a population of floscularias, but I was not seer enough to see what was before me then, as I looked for and thought only of nerves."—*Lancet*, December 9th, 1882.

The arguments against this doctrine are mainly negative, and may be summed up under three heads.

I. Clinical.—The assertion of the *de novo* origin of some 'so-called' specific diseases. This would imply under *unmodified* existing theories the *de novo* origin of specific disease germs, and involve the acceptance of the doctrine of abiogenesis.

The adherents of the Germ Theory meet this objection in one of three ways—(1) They look with suspicion on the accuracy of diagnosis and the consequent nature of the particular case alleged as an example; regarding it as a case of feigned or simulated disease. But it appears to me that when all the main characteristics of the typically generated disease are present, the calling of such cases "simulated" disease, is a *petitio principii.* The contention is that certain diseases can and do originate *de novo.* And when in support of this contention, cases are brought forward tending to prove it, it is objected that they are only "simulated" diseases, because they have not the essential element of production from infection—*i.e.*, because they have arisen *de novo*, the point to be proved!

(2) Where such alleged instances of *de novo* origin are comparatively common, as in Typhoid Fever, Diphtheria, Erysipelas, &c., they are inclined to let the diseases in question be relegated to the catena of "non-specific" or quasi-

specific diseases.

(3) They argue with respect to all Zymotics as Professor Corfield does regarding Typhoid Fever, that—"From the fact that we are able so frequently to point out the source whence the contagion has been derived, and to trace it to a previous case, we have at any rate a strong presumption of the view that when isolated cases occur, and there appears to be no connection with previous ones, the fact is that we are unable to trace any such connection, and it is clear from the nature of the disease that this must very frequently be the case; and moreover we have no right to assume that we even yet know all the methods by which the poison of this disease may be conveyed."*

I shall show, I trust, farther on, that there is another aspect of the Germ Theory which is perfectly consonant with all existing requirements, and which will meet this as it will meet all other difficulties, by fairly explaining them without denying their existence in the face of evidence.

II. Pathological. It is urged that the organised nature of the poison has never been conclusively demonstrated. Organisms it is true are admittedly found in the blood, tissues and secretions of patients labouring under the disease; but then similar organisms have also been found in the healthy subject, and moreover though diligent search has been made, it has failed at times to establish their presence in disease, while their relation thereto whether that of cause or effect, is still in debate even among those who clearly admit some kind of connection between them.

---

*Corfield: On the alleged spontaneous production of the poison of Enteric Fever. Reprinted from the Transactions of the Epidemiological Society, March, 1874.

In reply to this may be adduced the experiments of BRAUTLECHT and EBERTH who cultivated the Typhoid Bacillus and by inoculation produced an artificial Typhoid with inflammation of Peyer's Patches in rabbits; also the frequently repeated experiments of PASTEUR, GREENFIELD, and others, with the *Bacillus Anthracis*, by which successive cultivations of the organism in a suitable nitrogenised medium were shewn still to retain their disease producing attributes; and many other experiments of a like character and purport.

III. General. And finally it is objected that there is no need to formulate any such hypothesis. For the conditions and phenomena resulting from the presence in the body of the decomposition products of complex chemical compounds, abundantly prove that simple physico-chemical causes are perfectly capable of producing results similar to those presented by the Acute Specific Infectious Diseases.

To this it may be replied that the fact of this hypothesis being adequate to explain the phenomena, does not prove it in the absence of confirmatory evidence, to be the true explanation. That it is a matter of opinion whether it is more probable than or even equally antecedently probable with the other; and that if the other evidence in favour of the Germ Theory is as strong as I conceive it to be, the value of this objection would be *nil.* *

It appears to me that the strongest arguments against the "Germ Theory" as at present received, are rather those based on clinical grounds than those derived from Pathological considerations. And it is notorious that many Medical Officers of Health, especially in scantily populated rural districts, are yet holding their judgment in abeyance in consequence of the difficulties presented by certain clinical peculiarities. The question of the *de novo* origin of not only Typhoid Fever and Diphtheria but even of Scarlet Fever, and yet more markedly specific diseases (*e.g.*, Small Pox, as in my case related in Chapter I.), might possibly be disposed of by the arguments instanced by Professor CORFIELD if they stood alone. But unfortunately for the current theory there exist in addition certain classes of anomalies, which as a reinforcement to the arguments derived from the assertion of a *de novo* origin, form a very formidable phalanx indeed ; and to the consideration of these anomalies we shall proceed in the next chapter.

---

*There is a further argument which has been advanced by Dr. MOXON when enunciating his "sperm and germ" theory. He says:—"If this view seems to you more fanciful, and supported by less analogy than the fermentation theory, I will at once put before you what is a serious difficulty in accepting the fermentation theory of Fever. On this theory, is it not very difficult to explain how it comes to pass that one attack of a contagious fever protects from the danger of that fever ever after?" He then proceeds to ridicule the idea of that result being due to the organisms exhausting some pabulum essential to their existence, and asks what component part of the body can be so essential to Bacterial life, and yet so unimportant to the life of the animal that the latter can bear its complete destruction with impunity, and never renews it.

There is however another way in which immunity may be produced, viz., by the induction of a change in the functional activity of the tissue cells resulting from the struggle for nutriment between these latter and the Bacteria. To this view I shall have occasion to refer at greater length farther on.

CHAPTER III.

We have now to consider the question of anomalies as affecting the "Specific Germ" theory. And I propose to divide these anomalies into three types which will, I think, cover all conceivable cases. These three types are—

(1) **Variability in transmission,** or cases where disease of one type appears to convey disease of another type;

(2) **Diversity from unity**—Cases where from a common source, whether that be a disease poison (in which case it would also partially fall under type 1), or some insanitary condition, two or more distinct diseases appear to take their origin;

(3) **Nondescripts**—Cases of a character so indefinite that it would be difficult to class them; or which, though being in many points characteristic of some typical disease, yet present features of an anomalous kind, or such as belong properly to other diseases.

Arising out of this class we may consider the question of **Progressive Development,** *i.e.*, a series of cases at first indeterminate, or consisting merely of some symptom which is prominent in one of the ordinary specific diseases, but becoming increasingly more and more typical until at last we behold a distinct evolution of specific disease.

Since first my attention was drawn to the subject, by the extraordinary case related in the first chapter, I have lost no opportunity of collecting evidence on these points, and have even ventured to address a circular to a large number of members of the Profession, asking for information on these points. As I shall publish the replies received in an appendix to this volume, with an analysis of the answers, and with some remarks thereon, I will not here mention them farther than to offer my sincere thanks to those gentlemen who have so kindly acceded to my request.*

With regard to these anomalies, it would probably surprise many who have not paid special attention to the subject, to find out how exceedingly common they are. I am the more impressed with this conviction from the number of letters I have received, saying that the writer has never paid any attention to the subject, but that he is sure he has seen many examples if he had only noted them at the time sufficiently to be able to recall them to his memory. I hope I shall not be considered presumptuous if I say that this want of observation arises from a faulty method of thought. We are so accustomed to think and to speak of the "specific"

---

*The replies, I regret to say, have so far been too few to make it worth while to publish them *in extenso* at present. I have therefore merely analysed them, giving results of the analysis.

diseases, having in our minds' eye certain types which are beautifully described in the text books, that we refer everything to these types. "Clinically," it is said, "the diseases are specific. Small Pox arises only from and occasions only Small Pox, and so on with the rest of the class. Each of these diseases has a distinct type, to which the cases conform wite a wonderful regularity; and if aberrations do occur, they are very few in number and insignificant in importance being merely the 'exception that proves the rule.'"

But it appears to me that this is clearly a *petitio principii*, because it is **just the vagueness and indefiniteness of these anomalies which render their true nature liable to be overlooked** or misunderstood, and which make a numerical estimate of them, therefore, in the highest degree fallacious and misleading. For example, it would be easy enough to number up the class of thieves, and to state the numerical strength of that class, when only those who are detected is referred to; but it would be practically impossible to compare with this class the number of undetected thieves, for the very reason that they are undetected.

This fallacious argument appears to be due to the fact that having arranged a beautiful artificial clinical classification of diseases, the tendency not to depart from it is so great, that when cases at all abnormal present themselves, there is a strong inclination to **sum up the total of**

typical characteristics, and ignoring the anomalies, to "give them a name" by pigeon holing them under the already existent type which they most closely resemble. This fact has been very clearly pointed out by Dr. Hughlings Jackson, who in a paper published in the *Medical Times and Gazette* of August 15th, 1868, wrote :—

"The old method of investigation deals with circumscribed entities. It *looks* simpler. Thus many students are interested in being told that a case is one of *true* epilepsy, or of *real* chorea, or of *genuine* aphasia, who shew no interest in the description of a paroxysm of convulsion, not even the simplest ; who have never tried to form a clear conception of the sort of movements they can see in a choreal child, and who, whilst they are interested in such discussions as to whether a particular case is one of *genuine* aphasia or not, take no pains to obtain a realistic account of what that patient's condition was, what he really could say or could not say. *They have been taught to study cases as the symptoms shew approaches to certain clinical standards, and not as they shew departures from healthy states.*"

How applicable is every word of this to the customary method of regarding all cases of the so-called specific diseases, especially those belonging to the zymotic group !

But we may at any rate gather from the greatly increasing number of such recorded irregularities, now that attention has been for some time prominently called to their existence, that they form a much more numerous class than has hitherto been supposed; and it may not be long ere the variety of manifestations adopted according to circumstances by each germ, in the process of its specific evolution is recognized as so great, that the so-called 'typical' cases become a very insignificant minority. Our existing arrangement of these diseases will then be recognized as a purely *artificial classification*, founded merely upon obvious accidental resemblances, and will have to give place to a more *rational* one with less definite boundaries, just as the Artificial Botanical System of LINNÆUS has had to retire before the more scientific natural classification of DE CANDOLLE and LINDLEY.

We may now consider in detail the various classes of anomalies above referred to, merely in this place selecting a few typical examples. In the appendix will be found a list of many such published records extracted from various Medical Journals and other sources.

Class I. Variability in Transmission. DR. MEREDITH, Medical Officer of Health for Wellington Sanitary District, records the case of a servant girl who, having probably been exposed to the infection of Diphtheria, and having suffered from a distinctly Diphtheritic sore throat, infected a child with Scarlet Fever. The Scarlet Fever assumed a more pronounced type when transmitted to a younger sister, and though milder, was equally typical in a brother who also contracted it. The mother then fell ill; but she had had Scarlet Fever in early life, and her illness consisted of sore throat and systemic sypmptoms resembling much more closely those of Diphtheria than those of Scarlet Fever.—(*Lancet*, May 26th, 1883.)

In the *British Medical Journal* of February 1st, 1879, Dr. HOLLAND records a case where a pupil at a school contracted "decided Scarlatina" from two new pupils, who "though doing ordinary school duties, had a nasal discharge and labial excoriation attributed to cold, and were pale, asthenic, and a little feverish." "During the second week, the pupils fell ill in rapid succession to the number of 24 or 25, and the school was broken up. The invalids presented every *conceivable variety of Diphtheria and Scarlatina*, with and without rash. A. was a typical case of Scarlatina with the usual throat and skin manifestations and subsequent desquamation; B. was similar plus limited false membranes in the pharynx; C. was equally typical of recognised Diphtheria, with false membranes, albuminuria, sequential pallor, and paralysis, but with no discoverable rash."*

The following paragraph is quoted from the *Lancet* of

---

* For this and several other cases recorded I am indebted to Dr. DE GORREQUER GRIFFITH, who called my attention to them.

Oct. 27th, 1883, from the Reports of Inspectors to the Medical Department of the Local Government Board, p. 744. Its bearing on the point before us will be at once evident:—

"Dr. Parsons inspected the Thorne Registration District on account of an exceptional and long-sustained prevalence of Scarlet Fever and Diphtheria. During the prevalence of the former disease Diphtheria appeared, and the relation between the two diseases—a relation which has often been observed before—was evidently a very close one. There was no mistake as to the diagnosis of the two affections; the Scarlet Fever was the genuine disease, and the Diphtheria was marked by a thick, false, diphtheritic membrane, and was followed by the local paralyses so distinctive of that disease. And yet there were repeated instances in which the two diseases seemed to concur, or **even to be interchangeable**; indeed, **what was called Diphtheria appeared to have been contracted from a case called Scarlet Fever, and *vice versa*,** different members of the same family suffering simultaneously from the two affections, and in four fatal cases the patients being certified as having suffered from both. Dr. Parsons expresses the view that the disease tended more to assume a diphtheritic character in the case of houses where there were local insanitary conditions, such as defective and untrapped drains and foul middens, and he then proceeds to detail certain cases which appeared to illustrate the intercommunicability of Scarlet Fever and Diphtheria.

One of these cases may be taken as typical of the rest. A little girl, two years of age, child of a printer, in the latter part of September, 1882, had a typical attack of Scarlet Fever. She had associated before her illness with another girl, who was then sickening with what proved to be an attack of Scarlet Fever. The printer's apprentice, aged seventeen, worked in a room in which the little girl, after her recovery, used to come. He was taken ill on Nov. 6th of Diphtheria, of which he died on Nov. 13th. His mother, at whose house he resided, subsequently suffered from Diphtheria, followed by Paralysis of the legs and pharynx. In the house in which they lived was a scullery sink with a broken bell trap."

The relation of Scarlatina to ordinary Septic Fever is well shewn in what is termed "Surgical Scarlatina," an admirable account of which has been given by Mr. HOWSE in the *Guy's Hospital Reports*, vol. xxiv.; and the relation of specific infection to Puerperal Fever was shewn by Dr. BRAXTON HICKS in a paper published in the *Transactions of the Obstetrical Society*, vol. xii., in which he "called attention to the rose rashes occurring in Puerperal Fever, and has connected them with sources of infection from Scarlatinal cases, &c. But these cases nearly all *terminated in the ordinary symptoms of Puerperal Fever*, and the object of this very important paper was mainly to shew how ordinary Puerperal Fever arose from sources of infection of zymotic origin. Thus he has proved that not only the contagion of Scarlatina, but also those of Measles, Diphtheria, Erysipelas, Typhus and Typhoid, gave rise to Puerperal Fever."—(Mr. HOWSE's paper *supra.*)

Moreover, in his remarks on this subject on the same occasion, Dr. PLAYFAIR said—"In Dr. HICK's paper there are no less than seventeen examples, if I rightly remember, of women who had a disease which presented no symptoms of Scarlet Fever, whose symptoms were clearly due to the contagion of Scarlet Fever."

Class II. Diversity from Unity. This class of cases is more common than the first, and examples of it are more abundant. Instances are on record of the same insanitary conditions having given rise to Diphtheria and Typhoid in different patients; of Typhus and Typhoid originating from the same source; also of Scarlet Fever and Diphtheria, as, indeed, is described above; of Typhoid and Pneumonia, not to mention the more ambiguous correlatives of Sore Throat and Scarlet Fever, Sore Throat and Diphtheria, Diarrhœa and Typhoid, Diarrhœa and Cholera, Catarrh and Measles, Catarrh and Rheumatic Fever, and many others.

Beginning with the general question, I may refer to a passage in Dr. CHURCHILL's book on the Diseases of Women. He says:—"We find that the same seasons give rise to Erysipelas, Typhus, Scarlatina, and Puerperal Fever, that they prevail epidemically at the same time; and as an epidemic take on the same type, and appear capable *the one of giving rise to the other*, or of co-existing in the same patient."

Again, Dr. DICKINSON, Chairman of the Committee on Croup and Diphtheria of the Royal Medico-Chirurgical Society, on May 13th, 1879, said in reference to the discussion on this subject:—"There was evidence that Diphtheria was interchangeable with follicular tonsillitis; and he thought that it had also distinct pathological connection with other diseases. He referred to a series of cases in which poisoned water had been drunk, the results being in various cases, Diarrhœa, Purulent Ophthalmia, Erysipelas, Pharyngeal Diphtheria—all arising from the same cause."—*British Medical Journal*, May 24th, 1879.

In a clinical memorandum in the *British Medical Journal* of Jan. 25, 1879, p. 113, Dr. CLEMENT DUKES says:—"That the same poison which originates Typhoid Fever often causes Diphtheria; that the two diseases often occur side by side, in different individuals, in the same house; and that therefore we must look for their same source of origin, the following cases will show:—

"(1) On October 30th, 1875, I was called in consultation some distance in the country to a large private School, at V——, to see Mr. R., who was supposed to be suffering from Typhoid Fever, though he was not. There were, however, five or six cases of Typhoid Fever in other rooms that I saw. While these cases were in progress, and within a few days of my visit, a younger child developed Pharyngeal Diphtheria and died. These cases occurred at a large house standing by itself in several acres of ground; they were traced to the well water (there was no other water supply); there had been a great rainfall, the cesspools had filled and overflowed, and the well being near and on a lower level than the cesspools, the overflow had found its way into the well, which was marked by making the side of the well at which it entered slimy and blackish.

"(2) At the beginning of January, 1875, Mr. T., of Rugby, developed Diphtheria, which was followed by Paralysis. In the same house, on January 15th, 1879, Mr. T.'s servant sickened with Typhoid Fever, went through a severe illness, but recovered. During the last days of December, 1874, there being something amiss with the pipes which carried the water supply from the town to the cistern, the water was *cut off for one day*, to remedy the defect. On that day a pump in a yard at the back of the house was used for their water supply; this well was much fouled from adjoining stables. There was, however, another possible source of poison; for the trap at the water closet was defective, and allowed an escape of sewer gas into the house. Both sources of poison were remedied, all the family were sent out of the house, and we had no further cases."

So much for Diphtheria and Typhoid.

In a report by Dr. MEREDITH, Medical Officer of Health for Wellington Sanitary District, that gentleman records a case where the same insanitary conditions originated Diphtheria in one patient and Septicæmic symptoms in other members of the same family. An epidemic of Diphtheria ensuing, the later cases became mixed with Mumps, the Mumps becoming more pronounced as the Diphtheria faded away.

In his "Lectures chiefly Clinical," Dr. KING CHAMBERS devotes a considerable space to what he terms "Typh-Fever." That is a term invented by him to cover both Typhus and Typhoid, the clear distinction between which in their more typical forms now universally recognised, was then an open question.

On page 96 he relates the following:—"Case 15. Edward P. (Case Book 103, p. 114.) This robust man was again one of a set where the fever was traceable to a poisonous locality. You may remember that he made so much noise by yelling "Dust ho!" that he was obliged to be removed to the noisy ward. He turned out to be a workman at a neighbouring dust contractor's, and the foreman of the yard was brought in a few days afterwards in Dr. ALDERSON'S week; and a third dustman came from close by and will appear as case 19. All these were very muscular men, and very noisy. The first and second had rosy raised spots, which were peculiarly distinct and characteristic in Dr. Alderson's patient; but the third had a livid rash; and in the first, who is at present under review, there were livid mixed with rose spots."

Again:—"Case 20. Stephen F. (C. B. 103, p. 246.) I hear from Mr. SANDERS, of Chigwell, who attends this boy's family, that they had Fever with "mulberry rash" in the house, and that a brother had died of it. Stephen was sent to London to be out of the way, but sickened on the day of his arrival, and had rose spots in several crops." *Idem.* p. 98.

The same writer also affords me an instance of another combination: "Edward C., a boy of seven years old, was seized on Dec. 28th with rigors, nausea, loss of appetite, nocturnal delirium, and febrile depression, so that he could not lift up his head. He was brought under my care on December 31st, by which time there were numerous dusky fever spots on the belly. He was put upon the usual treatment, and is now convalescent. . . . . It is remarkable that a few days before the boy was attacked by Typhoid

Fever, without any local complication whatever, his father fell ill, and was brought into the Hospital on Dec. 29th with Pneumonia affecting between one third and one half of the left lung. The father has had no delirium, nor more nervous depression than is accounted for by the condensation of the lung, no eruption on the skin nor diarrhœa, nor anything which would lead one to set down his complaint as due to the same cause as his son's. Yet the coincidence joined to the fact of their residence being an ill drained, ill ventilated house, where almost everyone has been ill, is too striking to be passed over in silence. Did the exposure to cold and wet during his daily labour predispose the hard worked father's lung to be affected by the same cause, the typh poison, as absorbed into the boy's blood acted principally on the nervous system? Was his more robust frame, and the *dura messorum ilia* able to pass unscathed a dose of the typh sufficient to poison the more delicate tissues of the younger subject, while at the same time his lung, devitalised by wearing toil and exposure, yielded to its influence? Is, in short, the Pneumonia the expression of a quantity of poison enough to injure a failing part, but not enough to affect the whole body? I do not like to answer these questions *ex cathedrâ*, but I cannot help seeing some connection between Typh Fever and Inflammation of the Lung."

In respect of the explanation of this case, I should say rather that the solution of this matter must rest entirely upon the question of constitutional and local predisposition, and not upon the *quantity* of the poison, which, as we know, is indefinitely reproduced in the system; that while the constitution as a whole was sufficiently vigorous to be enabled to shake off the remainder, the weaker and less vigorous lung succumbed to the attack.

I have already referred to the apparent origin of Scarlet Fever and Diphtheria from the same source in the case quoted from Dr. HOLLAND; and I will therefore only add here when Dr. HOLLAND conveyed the infection home to his own family, eight took it; the eldest, who had previously had Scarlatina, resisted well, while the youngest two contracted "malignant Diphtheria as bad as could be, short of actual destruction; they had no rash, but false membranes through the pharynx, nose, and mouth, albuminuria, sequential pallor, paralysis, and quasi-idiocy; and three years of careful treatment have scarcely restored their pristine vigour." (*The Propagation of Diphtheria by mild cases and disguised combinations.* E. HOLLAND, M.D., M.R.C.P., *British Medical Journal*, Feb. 1st, 1879, p. 148.)

Class III. Nondescripts. It is not, I should imagine, requisite to say much on this score. Doubtless, most practitioners have met with cases of febrile disorders (some of them infectious) of an indeterminate type, and which have sorely taxed their diagnostic ingenuity. I had myself, in 1881, many cases of an acute febrile disorder, which for want of a more accurate diagnosis I was compelled to denominate Rötheln;* though all of them were of very irregular type, some leaning strongly to measles, others to Scarlatina. The case first quoted in this book is an example of this class, as were also some of the cases in the School Epidemic reported by Dr. HOLLAND, and the dustman referred to by Dr. KING CHAMBERS, who developed a mixture of rosy and mulberry spots.

In a review published in the *British Medical Journal*, of January 5, 1861, on Dr. GREENHOW'S Book on Diphtheria, referring to the epidemic of that disease, which appeared in England in 1856-7, it is remarked:—"A curious coincidence here has been suggested; viz., that during this visitation of Diphtheria other diseases (especially those of the zymotic sort) assumed a diphtheritic type, and not only amongst men, but also amongst other animals."

But more recent instances than these are forthcoming. In a paper on "Some peculiar features of a recent Epidemic of Measles at Cheltenham," read before the Section of

---

* As I find I have been misunderstood on this point, I deem it necessary to state that I do not regard Rötheln as a hybrid between Scarlatina and Measles, but as a distinctly 'specific' disease, so far as these diseases can be said to be specific. But the lower we go in the scale of life, the less is the intension of the term specific.

Public Medicine, at the Worcester Meeting of the British Medical Association, in 1882, Dr. E. T. WILSON, after enumerating a series of peculiar symptoms, calls attention to their close conformity to Rötheln, and their difference from ordinary measles. "The short duration of the premonitory symptoms, the prominence of throat trouble rather than coryza, the peculiar character of the eruption and its early appearance, the swollen lymphatics, the peeling, as well as the almost universal constipation, whilst they are very characteristic of Rötheln, are certainly unusual in an unmodified epidemic of Measles. On the other hand, the symptoms supposing them to proceed from Rötheln were unusually severe for that disease, although there are not wanting instances of its occurring in a very aggravated form. . . . . On one point, however, there is the widest possible divergence. Whilst in Dr. CHEADLE'S cases (*Trans. of Internat. Med. Congress*, Vol. IV., p. 4 and sqq.), a previous attack of Measles proved no protection (out of 30 cases 22 had the Measles); among the inmates of two large boarding houses, during this epidemic, a previous attack of Measles apparently gave absolute protection."—*British Medical Journal*, September 16th, 1882.

At the same meeting, Dr. BOND, the Medical Officer of Health for Gloucestershire combined Sanitary District, said, in reference to an epidemic of Scarlet Fever, that "whilst in its typical forms as easily recognisable as any disease, it shades off so imperceptibly into Measles on one side, into Diphtheria in another, and into ordinary Catarrh in a third, that it is practically impossible, so far as my experience goes, to say where the line of demarcation is to be drawn."

I will here refer to the existence of Scarlatiniform rashes in Puerperal und Surgical Fevers, in Enteric and Rheumatic Fevers, and occasionally in Small Pox, also after the eating of shellfish, tomatoes, &c.; as well as to a case of Measles rash in Typhoid Fever, recorded by Dr. W. C. Beevor in the *British Medical Journal* of May 19th, 1883. A further list of similar recorded anomalies will be found in the Appendix, and I need not therefore here dwell longer upon that subject.

It has been noted by others that these phenomena to some extent cast a doubt upon the theory of "Specific Germs," and so far upon the Germ Theory of disease as commonly understood and taught. That theory, briefly stated, is as follows:—That the acute specific infectious diseases are due to the workings in some way or other in the animal body of perfectly distinct and functionally specific microorganisms. That each microorganism can cause its own disease, and that alone; and that whenever we find a case of any one of these diseases, we are bound to infer that the patient has directly or indirectly been in contact with some person or thing infected, whence the specific microorganism has gained entry into his body, and instituted the series of phenomena which make us cognisant of the presence of disease.

As a commentary on this view, I quote the following remarks by Professor HUETER, of Griefswald, at the International Medical Congress, at London, in 1881. He said—"Practically, the question of the specific functional activity of microorganisms and their relative unity, may be stated something in this way:—Is it necessary that a person who falls ill with the Scarlatina, Measles or Variola, must have received the infective material from another patient affected with Scarlatina, Measles, or Variola? I for my part answer the question with "No!" while the defender of the theory of absolute specificity must answer it in the affirmative. I consider it possible that at any spot the putrefactive processes may take on such a course that microorganisms arise from them which produce Scarlatina. The disease might then originate at this spot without there having been any absolute continuity of infection from another case of Scarlatina."—(*Transac. Int. Med. Cong.*, vol. i., p. 329.)

It will be seen from this passage that Professor HUETER was by no means so certain of the Clinical questions involved in this enquiry; and that he inverted the process of thought by reasoning from the nature of the germs to the Clinical aspects of disease, instead of from the Clinical aspects of disease to the nature of the germs.

On this subject, too, Professor STOKES, in a "Discourse on State Medicine," published in the *British Medical Journal* of April 13th, 1872, says:—"The laws of epidemical and of endemical diseases, and the manner in which they arise have yet to be worked out; so also the correlation, and, perhaps even the convertibility of zymotic affections. This bears on the "Germ Theory" lately revived, but as yet not handled with the modesty of Science. 'Does like in disease always produce like?' is a question to be asked. The whole subject of contagion and the application of the doctrine of probabilities to its proof and disproof; the variations in the state of receptivity of the body; the influence of the law of periodicity on diseases, whether as regards the world wide pestilence or the individual case—all require elucidation."

And again, he says:—

"No one who has not had a lifelong experience of epidemics, can estimate the difficulties which exist as to their origin, as to the absence of essential fever in places where, theoretically, it ought to prevail. The appearance of epidemics at irregular periods, while their supposed exciting causes remain constant; their disappearance, though the causes continue in full operation; their outbreaks in all latitudes, climates, and seasons; their different modes of spreading; the want of constancy in their symptoms and history, for every great epidemic has its own character; the varieties as to the extent, nature, and effect of the secondary affections which arise in their course; the varieties of their mode of subsidence and behaviour under treatment; their degree of mortality and contagiousness—all these things constitute the difficulties which surround us in our investigations as to zymotic disease. They bear on the supposed specific or constant origin of disease, on the error of drawing hard and fast lines between essential affections, and are with difficulty reconcileable with the Germ Theory."

In a debate (referred to above) at the Obstetrical Society of London, in 1875, "On the relation of Puerperal Fever to the Infective Diseases and Pyæmia," Dr. PLAYFAIR is reported to have said, "We are only at the threshold of the enquiry, and have to build up all our knowledge by unprejudiced and patient clinical investigation. The relation of the Specific Zymotic Diseases to the so-called Puerperal Fever is one of the most obscure subjects, and *most difficult to reconcile with the Germ Theory* of any question connected with Puerperal Fever, and cannot be got rid of by simply denying it."

Again, in a paper on the "Ætiology of Typhoid Fever," published in the *Medical Times and Gazette* of August 2nd, 1879, by Dr. ROBERT KING, Physician to the Middlesex

Hospital, that gentleman says:—"I cannot but regard the theory of *contagium vivum* as highly dangerous in the hygienic point of view, since it practically excludes the possibility of diseases such as Typhoid Fever being generated *de novo*, for the only alternative would compel us to admit the unphilosophical doctrine of heterogenesis."

And again:—"If, then, we can divest ourselves of the notion that the typhoid poison is a living entity, propagating its kind by one or other of the various methods peculiar to vitalised bodies, I think there is no great difficulty in assuming the possibility of its arising *de novo*."

Many others besides Professor STOKES, Dr. PLAYFAIR, and Dr. KING, have doubtless felt their minds perplexed between the undoubted evidences in favour of the view that the so-called Specific Diseases are the result of the action on the animal system of specific pathogenic microorganisms, and the difficulty of reconciling that view with any adequate explantion of the anomalies of origin and character to which this chapter is devoted. I hope in the next chapter to shew that not only are such anomalies compatible with the notion that the typhoid poison, as well as that of all the infective diseases, is "a living entity propagating its kind by one or other of the various methods peculiar to vitalised bodies," but that these are just what we ought to expect, and are, in short, a powerful evidence in favour of the Germ Theory.

And this I propose to do by the simple application of the principle of evolution to disease, by means of the Darwinian doctrine of the origin of species, as affecting pathogenic organisms.

CHAPTER IV.

The general application of the great doctrine of evolution to disease appears to have been more or less distinctly "in the air" for some considerable time; though I must confess to being ignorant of the fact until after I had read my first paper on the subject before the Medical Society of London, in 1882. Moreover, as not infrequently happens in philosophical speculations, it appears to have come prominently forward at the same time, and independently, in the minds of many persons. In suggesting, therefore, the application of this doctrine to the "Specific" diseases, I am so far in common with many.

In the *Medical Times and Gazette* for September, 1881, Dr. W. H. Pearse, of Plymouth, published a communication entitled "Instances of the Evolution of Epidemics and the Alliances of some Diseases," in which, after recounting his experience at sea with regard to Cholera, Measles, Small Pox, Chicken Pox, and the mutual affinities of Measles and Scarlet Fever, he says:—

"Although the experience of this voyage does not *prove* the evolution of Scarlet Fever and Whooping Cough, yet it carries to me the deep conviction that, in viewing the phenomena of the normal rates or state of the human system (in the class of fevers or 'colds'), the alliances of so-called different diseases have a greater place in nature, or in their 'form,' than have their 'specific' differences." And further on, he writes:—"The facts of the Germ and Poison Theories will form minor parts of a grander theory of evolution."

In a letter to the *Lancet*, of May 14th, 1881, which I have thought sufficiently forcible to reproduce in the Appendix, Dr. W. H. COLLINS says:—"Since, however, as regards the organic world, we have come to speak of an origin of species, and learnt to trace their differentation under the influence of evolution, to the eventual survival of the fittest, I would ask, should not our views concerning specificity of diseases undergo a similar modification? Is not the theory of specificity swallowed up in the larger theory of evolution?"

More recent and elaborately worked out instances of the application of this doctrine, in one method or other, to disease, are to be found in the Bradshawe Lecture of Sir JAMES PAGET, "On some new and rare diseases," delivered last year (1882), and published in the *Lancet*, of December 16th, and other Medical Journals; and also in the Address in Pathology, delivered at Liverpool this year to the British Medical Association, by Dr. CREIGHTON, of Cambridge, "On the Autonomous life of the Specific Infections." To both of these I shall have occasion to refer bye and bye.

Now all these facts shew a tendency to apply the doctrine of evolution to disease. When I say, then, that I conceive that **any one of the specific diseases might arise** *de novo*, **as it must certainly at some period in the world's history have done for the first time, and that having so arisen, it is capable of transmitting its like by infection,** I merely assert what is the logical outcome of a position already more or less widely taken up. The chief objection to this is encountered at the hands of those who have espoused the "Germ Theory" of disease. If these diseases, it is said, arise *de novo*, then it follows that their special germs must arise *de novo*, in other words you must adopt the doctrine of heterogenesis.

To this I would reply "not at all." The evolution of specific germs, or rather the **acquisition of specific properties by germs,** no more implies heterogenesis than the doctrine of the "origin of species" by progressive development implies the theory of a specific creation. On the contrary, it is because I attach great importance to the weight of argument adducible in favour of the specific infectious diseases being due in some manner or other to the working of special microorganisms in the human economy, that I regard this doctrine of evolution as of the highest moment; for it, and it alone, so far as I know, can adequately meet the objections based upon clinical grounds. I claim for it two great recommendations—firstly, that there is no reason why microorganisms should be excluded from the operation of a law now almost universally admitted to govern the order of the universe; and secondly, that it is capable of adequately explaining the principal difficulties that beset the "Germ Theory" of Disease, viz.: those consisting in evidence of clinical anomalies.

I would here point out, what must be doubtless known to all, though none the less liable to be forgotten, that the idea of specific pathogenous bacteria is really the result of the idea of specific diseases, and has not arisen from any specific morphological characters (in the biological sense); for speaking generally, specific characters are not as yet clearly established as existing in the organisms associated with particular diseases, save by the clinical results of their functional activity. It is because we see different clinical phenomena, which grouped together in the aggregate, make us cognisant of the nature of the disease, that the adherents of the "Germ Theory" conclude that the germs must also be possessed of essential differences. But these clinical phenomena quite clearly do not reside in the bacteria themselves, but spring out of their relation with something else. Now, it is only reasonable to suppose that that something else—the human vital* processes affected—is not without influence in determining the character of the phenomena.

I have quoted frequently from Dr. CHAMBERS' "Lectures chiefly Clinical," and once more I recur to his words. On page 73, he says :—"There is every reason to believe that one of the chief exciting causes of these fevers is a poison generated by decomposing organic matters, and received into the body from without. To judge by its effects, it seems widely diffused through the air, especially in the neighbourhood of its origin in the air of sewers, putrid marshes, and crowded human habitations. . . You are tempted to ask how it is, if the poison is spread so broadcast, that everybody does not get poisoned. But remember, there are two things necessary to poisoning; *not only the poison, but a person apt to be poisoned.* And in point of fact, the latter is the most important element in the transaction."

Another way of expressing the same essential idea, viewed, however, with special reference to the "Germ Theory," is exemplified in the Bradshawe Lecture of Sir JAMES PAGET, to which I have already referred. He says :—

"Perhaps the brilliant success which has been achieved by the recent studies of disease-producing organisms, or other materials acting on us from without—a success not equalled in any other field of medical enquiry—has made some think too little of those changes within ourselves which occur in such ordinary conditions of life that they may be called spontaneous. Yet these are not less important in the production of diseases, and these must be studied; **just as in agriculture, soils must be studied as well as seeds.** This is true even in respect of those diseases whose essential causes are most evidently external, even of those which are due to specific contagia; their germs or seeds, if I may so speak, will not germinate in an unfit soil. I suppose there is not a day in which most of us do not inhale or come in contact with the germs of some frequent or contagious disease; but they do not germinate in us any more than do the seeds of tropical flowers in our streets, or in the fields to which the wind scatters them; we do not offer the fitting soil. **And even among those in whom they do germinate, the product varies according to the soil.** And the study of this soil, this living soil, is yet more necessary in respect of diseases which come in part or wholly by inheritance; for it is in each as personal and distinct as any other constituent of personal character, and the study of it must be intimately personal, with an exact analysis of every disposition to disease."

---

*I use the term "vital" not as implying any particular theory, but merely to represent the sum and resultant of physiological forces in action.

As a parallel to this may be cited a passage from a paper by M. BOUCHARDAT. "Sur l'hygiène et la thérapeutique étiologique," published in the *Bulletin Générale de Thérapeutique* of April 15th, 1883.

"Deux choses sont à considérer lorsqu'on cherche à apprécier l'influence des causes des maladies : 1° le modificateur extérieur, qui peut être un parasite, un ferment morbide, un poison, un agent physique, on chimique ; 2° l'individu qui reçoit l'impression de cet agent. Nous retrouverons bien souvent ces deux termes isolés, que nous pourrons désigner par deux mots empruntés à l'agriculture, la graine et le terrain, l'acarus et le galeux."

Again, Dr. Sansom, in his remarks on my paper "On some suggestions for a modification of the Germ Theory of Disease," read before the Medical Society of London on Feb. 27th, 1882, said :—"I consider the differences of soil even of more importance than those of organisms in determining the nature of the specific diseases. I have long considered that there is a balance of probability in favour of the view that the specificities of disease are rather a matter of cultivation than of morphological distinctiveness." *

---

* A clinical instance of this effect of soil is seen in the fatal results of Measles—by no means a malignant zymotic—when introduced into what may be termed the virgin soil of the Fiji Islander's constitution, as happened some few years ago. The mortality reached an enormous per centage. A similar result followed upon the introduction of the same disease some years back into the Faroë Islands.

The following passage is also to the point :—"Thus it follows that the less war, the more pestilence ; the greater the civilization, the more

Now, how can we apply this great principle of evolution to disease germs? Let us consider how, in all probability, the first specific infectious disease germ arose. Suppose that, according to the "Germ Theory" of Disease, specific diseases are due to microorganisms, which we may term Bacteria, using the word for convenience' sake, and in a purely generic sense, then unless we pre-suppose the spontaneous generation of life in a special pathogenous micro-organism, we must conceive a time when the ancestors of that organism were not yet disease germs. They existed as Bacteria, but were not pathogenous Bacteria. Now, what is the effect produced by Bacteria on the medium upon which they thrive? It is profoundly altered in two respects: first, by the abstraction of certain elements which the organisms have appropriated for food; and secondly, by the addition of their effete material which is cast off as the result of vital processes. A like result, it is to be remem-

---

terrible the disease. This accounts for the fearful mortality which ensues among our autocthonous and savage people, when infected by the diseases of a race much their superior in civilisation. Could a better instance be given than in the case of the Micmac Indians, annihilated by a single attack of Small-pox?"—LAWSON TAIT. "Has the law of natural selection by survival of the fittest failed in the case of man?"—*Dublin Quart. Journ. Med. Science*, Feb., 1869.

The strong hereditary tendencies occasionally shewn, and the powerful predisposition which would permit a man to have five well-marked attacks of Scarlatina before attaining his majority, as related by Dr. C. L. DANA, also form in some sort collateral evidence.

bered, occurs in the case of all organised beings, who deprive their environment of material for food, and contaminate it with their excreta.

Suppose by some mischance, one of these 'indifferent' germs, or non-pathogenous organisms, is transferred from its customary *habitat* in sewer or stagnant pool into the system of a human or other organised being, what will be the result? Obviously, that will depend upon its power of 'adjustment to its environment,' (HERBERT SPENCER.) Should that faculty be small, it will ultimately die out, after producing an effect more or less marked in inverse proportion to the resisting power of the vital processes of its new surroundings, or perhaps will perish without any perceptible result. But should that faculty, on the other hand be powerful, it will probably find and appropriate to itself a pabulum in the blood, upon which it can thrive and develop, and in time after propagating itself (*i.e.*, after a 'period of incubation') produce changes in the organism which express themselves in the phenomena of disease—*i.e.*, in a departure more or less characteristic from that due performance of every function which we call health. *

---

* Dr. THORNE THORNE has, I find, expressed views almost identical with these. In a paper read before the Epidemiological Society, he said :—"If the contagia of acute specific diseases do, as has been suggested by more than one observer, belong to the vegetable world, I know of no grounds for refusing to believe that organisms capable of

Now, in considering the relation between the organism and the clinical phenomena of disease, there are two principal hypotheses open to us as to the causation of the latter. While some contend that the phenomena are effected by the abstraction of something from the blood, others believe them to be due, not to what the organism takes away, but to what it adds, viz.: its excreta, or the results of its own vital processes. In support of the former view it is urged that:—

1. The **immunity against the disease** for the future which an attack of diseases of this class in general conveys, points to the exhaustion of something, the presence of which is necessary for the existence and multiplication of the disease germ. Later on I shall call attention to a much more probable explanation of the fact of immunity than this

---

producing a minor and incommunicable disease in particular stages of their growth, may, in other stages of their growth, or in the course of their subsequent (race) development, become capable of producing a major disease communicable from person to person; the affair being essentially one of soil. **This is not at all a question of the development of a living organism out of matter independently of antecedent life, but merely the production by means of a process of evolution, of that which gives to an already existing organism that property by which it becomes infective**—a property which it may, perhaps, lose directly it is deprived of the circumstances which favoured its development, in much the same way as special characteristics may be artificially developed in higher plant life, and be as easily lost again."

'exhausted pabulum' view.

2. The effect of *Torula Cerevisiæ* upon a sugar solution, is to split up the sugar into alcohol and carbonic acid. The process resulting in the fermentation of the liquid is **analytical not synthetical.**

3. Experiment has shewn that microorganisms **flourish luxuriantly in proportion as they have an abundant supply of oxygen; and that where that is not at hand in a free state, they will split up some oxygenated compound to obtain it.** For instance, it has been pointed out in the *British Medical Journal*, of Dec. 31, 1881, p. 1061, that when the *Torula Cerevisiæ* is cultivated in blood outside the organism, it deprives the red blood corpuscles of their oxygen, and at the same time decomposes the sugar which is found in blood, into alcohol and carbonic acid. Dr. GEORGE HARLEY has also demonstrated this fact (*Lancet*, June 4th, 1881), and says that the deoxygenated blood failed to take up oxygen in the air, and only did so with difficulty when exposed in a receiver to an atmosphere of pure oxygen.

4. It has been found that the **blood of patients far advanced in these diseases, is very frequently deoxygenated,** and is so far altered that it does not become oxygenated on exposure to air.

On the other hand it is quite immaterial to our present purpose if it be that which is excreted, and not that which is abstracted, which determines ultimately the clinical phenomena. For surely it is not only rational but in accordance with what we should expect, that the **excreta or secreta depend to a very large extent on the nature of the pabulum upon which the organism thrives.** For instance, NAEGELI has found that the *Bacterium lactis* which produces lactic fermentation when cultivated in milk, when grown in saccharated extract of meat, is so influenced by its environment, that it produces an ammoniacal decomposition. If it now be continuously cultivated in this medium it will acquire an hereditary functional action so strong, that it will continue, if retransferred to milk, to induce ammoniacal decomposition and not lactic fermentation; this property, however, dying away as the later generations become once more accustomed to their new medium. This is clearly an instance of the effect of pabulum or environment on the specific functional properties of a microorganism.

But there is no reason to suppose that both factors may not take their share as causative agents of the clinical phenomena of disease. This view has some colour lent it by an experiment of PASTEUR'S on Fowl Cholera. He found that whilst the microorganisms of that disease, separated by repeated cultivations from their original breeding ground in the fowl, reproduced the disease when injected into chickens, the injection of the cultivation medium (chicken broth), freed from organisms, produced the drowsiness which is one of the prominent symptoms of the disease. The effect, however, was only temporary, and passed off in a few hours without destroying the fowl.

ZIEGLER says, in his Manual of Pathological Anatomy, that "the pathogenous bacteria are parasites which draw their nourishment from man's body. They withdraw this nourishment only from the tissues among which they are growing and multiplying. The effect of this withdrawal is, in general, not very grave. It can only become dangerous to life when the bacteria multiply within the circulating blood, and withdraw from it the indispensable oxygen it contains. The withdrawal of nutriment is not, however, the only result; it is seldom even the most important. Investigations shew that the vital activity of the bacteria of necessity sets up extensive chemical change in their own nutrient materials. These changes are partly due to their direct action, partly to the action of the unorganised ferments* which they form. Lastly, in the course of these changes matters are produced which act as poisons upon the system. This effect of the bacteria on their nutrient fluids, and the production of poisonous matters, have much more to do with the genesis of the symptoms in most of the microparasitic affections than has the mere withdrawal of nutriment."—(*Manual of Pathological Anatomy*, Vol. I., p. 287. Translated by D. McAlister, M.D.)

---

* Prof. SIMON compares the relation between the ferment produced by the organism, and the organism itself, with that subsisting between the living gastric cell, and its product pepsin.—Art. Contagion.—Quain's *Dic. of Medicine*.

But whatever view be adopted as to the method in which the disease-producing functions of the organism are modified by its surroundings, the fact that they are so, is attested both on general principles and analogies, and by experimental facts.

On general principles we may refer to Darwin's laws of variation, and say with Dr. E. T. Wilson, "It will scarcely be contended that the specific organisms of infectious diseases are exempt from the ordinary laws of development and evolution. We should rather expect that, in common with the lower forms of life, they would illustrate them in an exaggerated form under varying conditions of soil and nutrition."*

Again, one of the great principles of Physical Science, the Correlation of Forces, bears to my mind a striking analogy to the application of the doctrine of evolution of diseases and disease germs, and suggests a 'Correlation of Diseases.' That a change of conditions may convert heat into electricity,

---

* "On some peculiar features of a recent Epidemic of Measles at Cheltenham."—*British Medical Journal*, September 16, 1882.

electricity into motion, and motion back again into heat, is *primâ facie* no more credible than the transformation which may be effected in organised beings under diverse conditions of existence.

A practical application of the doctrine of evolution is afforded by Fraulein MARIE VON CHAUVIN, who some years ago succeeded in evolving a land salamander, by a series of carefully regulated surroundings, from the water-breathing Mexican Axolotl. On this, a writer in *Knowledge*, of October 13th, 1882, remarks:—

"But nature herself, notwithstanding the difficulties experienced by Fraulein VON CHAUVIN in transforming her Axolotl (some of which died under treatment), apparently succeeds sometimes in evolving Amblyostoma from the Mexican newt; **therefore, assuming the variability of specific microbes under the influence of oxygen, there is nothing unusual in the idea* of the parasitic germs of epidemics being spontaneously evolved from harmless saprophytes under peculiar conditions of culture,** such as the presence of various gases instead of free oxygen. In that case ill-ventilated sewers, stagnant pools, and other places where aëration is not sufficiently carried on, may continually evolve new crops of specific disease germs."

---

* Enunciated by Dr. ROBERTS, at the International Medical Congress of 1881, in London.

Is it any farther stretch of the imagination, I would ask, to suppose that the process of **evolution may go on as well in as outside the human body?** I fail to see it.*

Again,—"The relation between milk and outbreaks of Enteric or Scarlet Fever, or Diphtheria, has been thought of as a mere affair of disease entering milk cans by the aid of water used in washing, or of hands used in milking cows. It may turn out the disease-producing milk has received some organism that, under other conditions of life, would have been impotent, or such organism may have been derived from the body of the cow herself. Indeed, the hygienist is coming to suspect that he may often have to do with **organised material not far developed from perfectly non-specific germs.**"†

For experimental facts, I may refer first to the experiments

---

*On this very point Prof. SIMON, C.B., in the article—CONTAGION, in Quain's *Dictionary of Medicine*, p. 292, says in a foot note:—"In our own more special field, Pathologists have already learnt that certain of the so-called 'morbid poisons'—the contagia of Erysipelas, pyæmia and tuberculosis—are intimately related to the common ferment or ferments of *putrefaction*; and that the most vehement of these contagia can be developed by the artificial culture of successive transmissions in the living body from the comparatively mild contagion of any *common inflammatory* process."

†Address by Dr. BUCHANAN, President of the Epidemiological Society.—*Lancet*, December 3rd, 1881.

of Buchner, of Munich, who claims to have shewn that the harmless *Bacillus fœni*, is merely a functional modification of the deadly *Bacillus Anthracis* with which it is morphologically identical.

Though much doubt has been expressed as to the soundness of these experiments, still their results appear to me equally probable with those of the converse experiment, viz.: the well-established fact of the attenuation of virus by the continuous cultivation of pathogenous organisms, which has been so extensively practised by Pasteur and others in Anthrax and Chicken Cholera; which may be equally claimed as an argument in behalf of my position. But in reference to Buchner's experiments, Professor Virchow said at the International Medical Congress in London that "Buchner had studied *Bacillus Anthracis*, and thought he had proved that the same plant might be altered in its properties by a method of treatment resembling domestication—a new application of the principles of Darwin in this distant department of Science. Buchner thought he could reduce *Bacillus Anthracis* by a series of generations developed under certain conditions, to a perfectly innocent plant, the hay bacillus. Again, taking the hay bacillus, he thought he could give it virulent properties, by a sort of 'undomestication,' *(bewilderung)*, making it *wild* again. Thus by giving the organism a special kind of soil, and a more vegetable diet, it might be made innocent; by a more nitrogenous diet, and under other circumstances, it might be made wild or malignant again. It was not yet possible to judge of the importance (accuracy?) of these observations, as they had not been confirmed by other experimenters; but he could bear witness that BUCHNER was a good observer, and he thought well of his theory. An assistant of Professor VIRCHOW'S, Dr. GRAWITZ, had also taken one of the common kind of hyphomycetæ, the *aspergillus* form, and converted it into a wild or malignant form."[*]

I need not here do more than mention the experiments of NAEGELI, referred to above, which appear to shew that the kind of fermentation producible by a given organism can be changed by cultivating successive generations continuously in a foreign medium. This effect upon microorganisms of a change of environment is also shewn by cultivating the yeast fungus on potato slices, with a free supply of oxygen.[†] It then develops luxuriantly with mycelium and hyphæ. And it has been shewn that even if it be cultivated in a fermentable medium, with an abundant supply of free oxygen, while it develops luxuriantly, it affects the medium but little in the way of fermentation.

---

[*] *Trans. Internat. Med. Congress.* Vol. I., page 322.

[†] The author of a leading article on "Bacteria and Disease," in the *Medical Times and Gazette* of Nov. 3, 1883, denies this fact, contrasting the beer yeast, which by virtue of centuries of cultivation in highly specialised conditions "has acquired a fixity of form, and cannot by any change of conditions yet devised be caused to revert to a filamentous mode of growth," with the *Mucor racemosus*, in whose case a reversion is readily effected. I think it necessary, therefore, to state that at the suggestion of Mr. LAWSON TAIT, who called my attention to this point in the early part of this year, I myself repeated the experiment referred to in my text, with complete success.

I may further refer to a morphological variety of the *Bacillus Anthracis*, described and figured by Dr. KLEIN, in the April number of the *Quarterly Journal of Microscopical Science*, as probably consequent upon conditions of cultivation and surroundings. He further says, "As the cultivation proceeds, say after several weeks, the number of chains and cells of the *Torula* variety gradually diminish, and the greater amount of the growth is composed of the typical smooth thin threads, with here and there an indication in them of a spherical or elliptical *Torula* like cell."*

A series of interesting experiments by Dr. GEORGE HARLEY, F.R.S., was published in the *Lancet*, of June 4th, 1881, which tend to shew, first—that some **non-pathogenous organisms are capable of producing disease**; and secondly, that the **virulence of such organisms can be greatly affected by the medium in which they have been cultivated.** The injection of *Saccharomyces Cerevisiæ* mixed with distilled water, into the left jugular vein of a dog, produced no symptoms, and when the dog was killed, there were no *post mortem* results. The injection of other vegetable moulds grown in decomposing vegetable soil (viz., green fungus from old wheat flour paste), also into a dog, caused temporary listlessness and drowsiness, indicating a futile 'struggle for existence' on the part of the germs. More marked, though not fatal, clinical results ensued when the same fungus was grown in an animal medium, viz., normal gastric juice. *Oidium albicans* grown in infusion of putrid muscle and suspended, after repeated washings, in distilled water, was still more marked in its toxic action; while "microscopic green fungi in full bloom from the scum on the surface of water, in a dish containing the putrefying intestines of a cobra . . . . washed in seven different quantities of pure water" killed a dog in forty-four hours. Upon this Dr. HARLEY remarks, "I came to the conclusion that the soil the fungus grows in and the food it lives upon have a material influence on its constitution and properties." Dr. HARLEY'S conclusions are sufficiently important to be reproduced here *in extenso*. He says:—"From the results of these experiments on vegetable organisms, the following conclusions were drawn:—

---

*Loco citato; p. 262.

(*a*) That certain kinds of vegetable germs are not deleterious—even when introduced into the circulation to animals.

(*b*) That certain kinds of vegetable spores can neither develop nor survive in the circulating blood of a healthy animal.

(*c*) A fungus, which may have no toxic properties when grown on one soil, may acquire toxic properties sufficient to be deleterious to animal life when grown in another.

(*d*) Certain kinds of fungus spores and filaments, when they produce pathological changes in the animal body, do so by virtue of their inducing chemical changes in the blood, and not merely by mechanically interfering with the vital processes.

(*e*) That the healthy animal organism possesses the power of destroying and eliminating certain kinds of living microscopic spores and filaments of fungi from the circulation." *Lancet*, June 4th, 1881.

I could wish for no better evidence for my views than that afforded by these experiments of Dr. George Harley. A non-pathogenous vegetable organism grown in one soil is incapable of producing phenomena in the animal body, apparently because the series of chemical changes induced in the non-nitrogenous medium of its cultivation can find no analogue in the animal blood, which being a living tissue resists its action, and, proving the stronger, ultimately overcomes and eliminates it. But **transfer this same organism to a nitrogenous medium already unstable and ready for molecular change, and the germ soon** learns to accommodate itself to a fresh mode of life, resulting in a fresh series of chemical changes, and one to which it will find an analogue in the animal body. The result of the struggle for existence is no longer the same, and this time the organism, not the animal, gets the best of the fray, for which the former has, so to speak, 'gone into training.'*

We find, then, that by a 'struggle for existence' under its new conditions, our hitherto non-pathogenous bacterium has succeeded in overcoming the resistant force of the animal into which it has effected an entrance—that force being the natural physiological tendency along the line of health, which every organism possesses more or less strongly. But in so doing, it has originated in the animal a departure from the normal physiological processes which produce the state of health, which departure constitutes a disease, and is evidenced by an aggregate of clincal phenomena whereby we recognise its presence. The 'struggle for existence' which was the lot of the first disease germ, however, is much mitigated in the later offspring (and here comes in the importance of regarding the matter not as one of time but as one of generations), which having propagated themselves indefinitely by a series of cultures in the body, have **acquired an hereditary tendency of selection and accommodation** enabling them to thrive with comparative ease on introduction into another human organism. This hereditary tendency, being more or less completely assisted by repeated similarity of environment, will naturally become stronger with each successive series of generations; and by the time the cultivation has been continued through several hosts—representing millions of generations—we find firmly established a specific disease organism tending always to originate the same series of changes, and so to reproduce the same disease, accompanied by the same aggregate of clinical phenomena. So far, our view has explained the development of the original disease germ, and has also given an answer to a question propounded by Dr. CREIGHTON in his address on Pathology, delivered at the Liverpool Meeting of the British Medical Association, this year. After referring to one "central and guiding principle in the doctrine of disease," the physiological principle, or the idea that "diseased states of the body are but modifications of healthy states, deviations from the beaten track, perturbations of the normal life, shortcomings of the physiological standard," he said—"But I am none the less confronted with the difficulty that a great deal of pathology appears to be quite different in kind from any physiology known to us. What are the physiological analogies of the infective and constitutional diseases, and how large a part of pathology do the diseases of that class stand for?"

---

* "The adaptation to a parasitic life must in all cases have been a gradual one, and, as there can be no doubt that parasitic pathogenic bacteria have been at some time or other derived from non-parasitic harmless bacteria, it would not be surprising if we should find in these simple organisms, so closely allied to the highly adaptable moulds (mucor, yeast, etc.), *some* which have both a free and a parasitic phase of life, according to the series of conditions into which they may be brought. Not only this ; but it seems almost necessary to suppose that pathogenic bacteria (assuming that others exist besides that of splenic fever and of fowls' cholera) have an intermediate phase of existence external to their hosts. It cannot be shown, for instance, in the case of *Bacterium Anthracis*, that the bacteria are passed directly from host to host ; apparently they exist widely spread on the surface of the earth. In this widely spread non-parasitic condition it is probable that they are harmless (as *Bacterium Subtile*), and that only here and there, from time to time, when definite conditions recur (such, for instance, as the admixture of animal fluids, blood, etc., with the more usual constituents of the soil), do they gradually, in those particular spots, undergo modification and become adapted to take the last step, and enter on a parasitic and disease-producing phase of activity."—*Medical Times and Gazette*, Nov. 3rd, 1883 (p. 516.)

I should reply to this question as follows:—Disease is the aggregate of deviations from the normal physiological line of health, and in the case of the specific diseases it corresponds to the aggregate of changes effected in the constitution and functional processes of the organism by the disease germs, and evidenced by more or less well known phenomena indicating interference with the central controlling faculty over the circulation, respiration, temperature, secretions, excretions, and nutrition, both local and general. And this reply would hold good whether these deviations be induced by abstraction of material from the blood, by addition of poisonous excreta thereto, by the organisms causing a mechanical impediment to the proper performance of function by particular tissues selected as their seat, or by any combination of these methods.

The question of the way in which the organism and its nutrient material are respectively affected is closely connected with that of subsequent immunity from contagion. As ZIEGLER points out (*General Pathological Anatomy*, Vol. I., p. 287), there is a struggle between the tissue cells and the bacteria, both of which possess catalytic powers, partly by their own fermentive action, and partly by forming unorganised ferments, and they sometimes even set up synthetic processes. "When bacteria proceed to multiply within a tissue, a double influence is brought to bear on the common nutrient medium; the bacteria enter into conflict with the tissue cells. We do not, of course, know in what exact way this conflict is carried on. But we may believe that the bacteria do not communicate to the organic compounds contained in the juices the same kinds of chemical motion as the tissue cells. They will not, therefore, give rise to the same chemical changes as the latter. More or less serious disturbance of the normal metabolism of the tissues must ensue. This is the first effect, but not the only one. Different kinds of fermentation cannot go on in presence of each other for any length of time. One is more and more repressed, and at length suppressed, as the other advances. This cannot, of course, happen without simultaneous injury to the corresponding ferment. Thus the prolonged presence of bacteria results in the suppression, not alone of the nutritive activity, but also of the other functions of the tissue cells; and at length their life itself is enfeebled or extinguished. . . . The issue of a bacterial affection is either the death of the patient, or the death and elimination of the bacteria. In the former case the bacteria interfere with the functions of cells so numerous, or so essential to life, that life becomes impossible. In the latter case the tissue cells gain the upper hand in the struggle for nourishment and existence, and the bacteria are at length deprived of the conditions essential to their continued life."

How well this all falls into place with the views I have enunciated! A "struggle for existence" has terminated by "natural selection" in the "survival of the fittest." What is the known result of the workings of evolution in such a case? Is it not that those organs, properties, or functions, which have proved useful in this dread struggle, become more highly developed in consequence, and are transmitted to the descendants in an intensified form? The properties which enabled tissue cells of the first generation to triumph victoriously over a bacterial invasion, will be a thousand times more powerful to that end when innumerable generations have each done their share towards the cultivation of these properties. Hence progress and immunity, till long disuse once more brings retrogression and susceptibility. Nor need we confine this reasoning to the individual only, but we may safely apply it to explain the extraordinary inherited immunity or susceptibility to disease which oftentimes runs through whole families.

Now what we have said about the evolution of the original disease germ holds good, I believe, at the present day. Not only are new diseases in constant course of development, but the old ones are constantly being redeveloped side by side with their more widely recognised transmission by infection. And in this constant evolution the diverse circumstances of environment do not fail to leave their impress of individuality, giving rise to reversion to former type, to hybrid diseases, to variability in transmission and anomalies of every shade and degree. It will be said that if this contention be sound, it must be equally true of the whole animal and vegetable kingdom, which it obviously is not; for the redevelopment of a horse or a cat, for instance, in the animal, or an oak or a rose in the vegetable world, is a thing unknown so far back as the history of man can reach.

The answer to this objection is to my mind both simple and complete. The higher the position in the scale of life occupied by any organism, the more complex are its conditions of existence, and the longer the period required for its transformation. In the lowest forms, however, of both animal and vegetable life, the conditions of existence are necessarily few and simple, whence it may be inferred that the conditions requisite to produce a specific variation are few and simple likewise, and so more likely to concur. There is, therefore, much greater probability of lowly constituted organisms of this character finding themselves placed under conditions in all respects similar to those which determined the origin of a species in byegone times, and thus by adjustment to their environment, once more developing into an already existent species. The probability of this event appears to me to be in direct proportion to the shortness of time occupied by the life history of the organism; or even if that be not strictly the case, at least we must confess that that consideration is one which brings the process of evolution within the range of human observation. We are surely more likely to be able to trace the process of development which would ensue under a varied environment, in the *Bovista Gigantea*, whose cells multiply according to Professor Lindley, at the rate of sixty-six millions per minute, than in the case of highly developed plants, which occupy a very considerable portion of time in their growth and development. The same remark will apply equally to bacterioid organisms, as compared with the higher classes of animals or vegetables.

I find that this same idea was actually enunciated by Dr. W. Roberts, of Manchester, at the International Medical Congress, in London. He said "that applying to them (the ordinary microorganisms of wounds) Darwin's theory of evolution, there was nothing strange in the idea that the ordinary organisms found in wounds might change their normal characters and become infective, leading to blood-poisoning in its various forms. By evolutionists **time was not measured by years but by generations.*** It was said that bacteria doubled themselves in twenty minutes, and that in a fortnight they had a thousand successive generations, which was equivalent to a thousand years in the life of the wheat plant, and to thirty thousand in the life of man."—(*Trans. Internat. Med. Congress*, Vol. I., p. 321.)

It appears to me, then, that it is the losing sight of this cardinal fact which is the source of the principal objections entertained to the doctrine of the Evolution of Morbid Germs. It is argued thus :—"These phenomena, which this view is to explain, occur under our very eyes, and to apply the doctrine of evolution for their elucidation we must suppose it to be a very rapid process indeed, in place of the slow, the immeasurably slow one that it is—so slow, that the limits of history can merely give us glimpses of it." But the slowness of the process is purely relative. It depends, as I have said, not upon any given length of time, but upon the number of generations; and to bacteria, a week means ages!

---

* "It seems clear that organic beings must be exposed *during several generations* to new conditions to cause any great amount of variation; and that when the organisation has once begun to vary, it generally continues varying for many generations." (Darwin: "Origin of Species," Sixth Edition, Chap. I., p. 5.) Not a word here about the length of time required, which is so persistently proclaimed by many—merely a question of "several generations," and the length of *time* will clearly depend upon the average duration of life in the particular individual under consideration.

CHAPTER V.

HAVING now considered the way in which the first micro-organism may be supposed to have become a 'disease germ,' we have only to bear in mind what has been said further back about the comparative simplicity of its conditions of existence, and the rapidity with which new generations, each in accordance with the ordinary laws of evolution liable to receive and transmit impressions and characteristics both hereditary and acquired, are produced, to see that such a combination of circumstances might easily recur and would have ample time to impress more or less permanent characters on the organism, thus tending to evolve *de novo* an already existent specific disease, by inducing the evolution of a specific disease germ.

The evolution of new diseases is undoubtedly going on in our midst and under our very eyes. This was the tenour of Sir JAMES PAGET'S Bradshawe Lecture, and the text was CHARCOT'S Disease and *Osteitis deformans.*

The question of the evolution of *Disease* in relation to the evolution of Man under the operation of the Law of Natural Selection by survival of the fittest, was ably dealt with by

Mr. Lawson Tait, so far back as 1869, in a paper on that subject, published in the *Dublin Quarterly Journal of Medical Science*, for February of that year. Indeed, in his "Descent of Man," the late Mr. Darwin refers to this paper, and even abstracts a portion of its argument.

But there occurs therein one passage which foreshadows the doctrine of the evolution of *Diseases*. Mr. Tait says: "Does it not (*i.e.*, the relation between disease as a means of natural selection and the progress of civilization with the consequent decline of other eliminating influences) account for the fact, that every century seems to have an epidemic more terrible than the epidemic of the century preceding, apparently in the same ratio as the difference of the condition of the people of the centuries in point of progress in civilization? Does it not account for the fact that no sooner has science overcome one epidemic, than another, far more formidable, makes its appearance? Did not the Elizabethan epidemic of Ague give way before Jesuit's bark, drainage and tree planting, only to be superseded by the more terrible Small Pox? This, in its turn, has almost succumbed before the genius of the immortal Jenner, only in its turn to give way to the horrible Cholera. Is it fair, under such circumstances, to accuse the Medical Profession of having stood still since the days of Hippocrates? Or need we be surprised at the evidence before us all of the existence of a change of type of something—be it of disease, or be it of human constitution?"

This change of type, this gradual unfolding of specific or idiomatic characters, appears to me, at least as likely an explanation of the confusion between Typhus and Typhoid, universal till quite recent times, as an unwarranted assumption of the incompetency of the keenest observers of a few years back.

But to return to contemporary evidence. Dr. CHATTERJEE, of Calcutta, tells me that a few years ago, Typhoid Fever was unknown in India. Observers who had been accustomed to it in Europe, some 30 years ago, were unable to trace its existence in the former country. Gradually, however, it made its appearance, no one knows how, and is now no longer uncommon.

A further instance is that of Yellow Fever, to which Dr. CREIGHTON referred in his above-mentioned address in Pathology. It appeared in the 17th century as a "new disease," following the Slave Trade, and is connected with the Negro race in its origin. It was regarded by Dr. AUDOUARD as a peculiar form of Typhus due to the emanations from the putrid dysenteric discharges of the Negro, and the fever owed its well marked specific type to the fact that the matters which excited it could be traced to the negro body. "There was something," he said, "quite peculiar in the Negro constitution, and it was not surprising that the discharges from his sick body should be able, when fermented, to produce in others a Typhus Fever of a peculiar type." In support of this view, Dr. CREIGHTON adduces in evidence, the fact that the Chinese, like the Negroes, do not take the Yellow Fever on shore, but suffer from the dysenteric and other non-contagious elements, which are the antecedents of Yellow Fever.

Here, it will be seen, we have a condition almost the converse of that referred to above, in the case of Measles among the Fiji Islanders, and Small Pox among the Micmac Indians. There a disease, elaborated in a white race, is appallingly severe and fatal in new soil: here the disease products of a dark-skinned race develop in the system of the white into a virulent specific disease.

The connection between Dysentery and Typhus may here be adduced, as well as the celebrated cases of Gaol Fever, in which prisoners brought up at the Assizes gave Typhus to the Judges and Counsel, though, as LORD BACON points out, the prisoners were not suffering from Typhus themselves. Dr. CREIGHTON further remarks:—"But we need not go farther back than twenty years ago, nor farther away than the city of Liverpool, for proofs of the *de novo* origin of a specific fever; no more striking proof was ever given of the genesis of Typhus out of dysenteric and other filth, than in the case of the Egyptian Frigate which came from Alexandria to the Mersey, in 1862, to be refitted. There were 400 Arabs on board, who had suffered a good deal from Diarrhœa and Dysentery; they had met with rough weather all the way from Alexandria, and the hatches had been battened down for two or three weeks continuously. When the ship arrived in the Mersey, the 'tween decks was in a disgusting state of filth, and the pilot who took the vessel up the river remarked to his wife when he went home, that that ship would be heard of again. He was himself the first victim, for he was seized with a fatal form of hæmorrhagic Typhus about a week afterwards. More than one hundred of the crew were on the sick list, chiefly from Dysentery, but it was carefully ascertained that **none of the cases were Typhus.** Most of the Arabs went to a public bath in their filthy state, and in a few days Typhus broke out among the bath attendants. Some of the crew were admitted into the Southern Hospital for various non-contagious disorders, and there also Typhus broke out."

"All the facts of this most interesting case were communicated to the Epidemiological Society by the late Dr. DUNCAN, Medical Officer of Health. It was Dr. DUNCAN'S opinion, and also Dr. CAMERON'S, the present President of the Medical Section, that the outbreak was clearly traceable to the Egyptian ship and her filthy crew, **but that it was**

not traceable to pre-existing cases of Typhus, whether on the voyage, or in Alexandria before sailing."*

Dr. THORNE THORNE, in a discussion on the Report of the Committee on Croup and Diphtheria, at the Royal Medico Chirurgical Society, May 13th, 1879, said, "he had investigated several outbreaks of Diphtheria, and in these he had found the earlier deaths referred to Membranous Croup, and the later ones to Diphtheria. The same thing occurred in the reports of Medical Officers of Health. As long as the exudation was limited to the larynx, and there was no evidence of any previous cases, the disease was called Croup; but when the disease spread, and the subsequent cases were plainly derived from the earlier ones, it was called Diphtheria. With regard to the Ætiology of the disease, he had made several investigations under specially favourable circumstances, in isolated districts, in which the earliest attacks were certainly not contracted from contact with persons having Diphtheria. Over a large area, too extensive for personal infection, there would be a great tendency to sore throat. Then groups of cases would occur, in which the disease was decidedly infectious; and later on there would be severe cases, with patches of membrane on the tonsils, and inflammatory symptoms, or with obscure inflammatory symptoms, difficulty of swallowing and loss of voice, but no visible exudation. At last, perhaps in a single village in the area, there would be an outbreak of Diphtheria in its severe and fatal form." [This is what I have referred to as Progressive Development.] "Hence the Diphtheria appeared to be developed from what was originally a simple sore throat, and the infectious character was gradually increased. When he met with cases of Membranous Croup forming part of an epidemic of Diphtheria, they always occurred in the beginning of the epidemic, and were also apparently less infectious than the later cases. This might be due to the circumstance that in the larynx the membrane was formed earlier than in the fauces, and death occurred before the infectious stage of the disease was reached. But it was possible that Diphtheria had a history and a pedigree, sore throat being at one end and Diphtheria at the other. Might not so-called Membranous Croup occupy a position in the development of the disease intermediate between sore throat and Diphtheria ?" *

---

* Dr. CREIGHTON'S Address in Pathology, "On the Autonomous Life of the Specific Infectious Diseases," delivered at the Liverpool Meeting of the British Medical Association, and reported in the *British Medical Journal* of August 4th, 1883.

---

* "Five weeks since I performed tracheotomy on a child for what the doctor and I took to be ordinary acute laryngitis. The child was *in extremis* from dyspnœa ; after the operation, all trouble ceased, and on the fourth day the tube was removed, and the child recovered rapidly. We were satisfied that the case was not one of Diphtheria ;

"He did not agree with Mr. HUTCHINSON in considering that Diphtheria was not one of the specific fevers. He thought that the diphtheritic poison was a specific one, but that it had not acquired the stability met with in the case of Small Pox, Scarlet Fever, Measles, &c. From time to time its specially infective property was, by a process of progressive development, brought to full maturity; and it then bred as true as Small Pox or Scarlet Fever. If this view were correct, it was immensely important to ascertain all that was possible as to the natural history of the disease, so that by effectual sanitary measures, the poison might be deprived of the conditions favouring its development. At present, the poison, even when developed, tended to revert to a former and more innocent stage. Unless controlled, it might some day acquire the persistency of some of the more stable poisons."—*British Medical Journal*, May 24th, 1879.

The following marked instance of the **evolution of Scarlet Fever by progressive development** came under my own personal notice :—

---

but within four days the father complained of sore throat, and quickly died of Diphtheria, in the paying wards of St. Thomas' Hospital. The drains of the house are, I believe, to be blamed for all the trouble. Dr. STOWERS, of 23, Finsbury Circus, knows more of the case than I do. My opinion is that possibly the child was poisoned from the drains, and that in passing through the child, the germs became developed into the toxic elements of Diphtheria."—Mr. EDMUND OWEN, F.R.C.S., in reply to my 5th Circular question.

During my absence at the Worcester Meeting of the British Medical Association in 1882, Mr. A. B. CLARKE, was summoned on my behalf, on August 11th, to see a little girl, three years of age, and found her to be suffering from Scarlet Fever. On my return, on August 13th, I saw her myself, and found slight sore throat, the child being decidedly better, and the remains of a rash. There was considerable swelling and tenderness of the left sub-maxillary gland, and there had been considerable fever. The patient subsequently desquamated freely.

The house was a lone farm house, some considerable distance from any other habitation, and was occupied by the father (a farm labourer), the mother, and four children. The situation was high, and on the whole, airy. There were the usual insanitary conditions of the farmyards in that district—middens with pumps near them, cow sheds against the wall of the house, &c.

Enquiry elicited the following facts:—The first case of illness occurred in a lad, aged 11. He had left school six weeks, and had not been away from home during that period. He had drunk some water from a well near the yard, and three days afterwards he complained of sore throat. He was said to have had no other symptoms, certainly no rash, and after three days the throat became all right. There was no noticeable desquamation.

A week after this lad was taken ill, the mother commenced to fail. She had rigors, which were followed by some slight feverishness, bad sore throat, which was still of a dusky and more or less inflamed character when I saw the first patient (the little girl first referred to), on August 13th. There had been a nasty purulent discharge from the nose; but there was no rash, and the case was not followed by noticeable desquamation.

Four days after the mother was taken, two more children —William, aged 15; and John, aged 5—were attacked. William complained of headache and sore throat; he had some diarrhœa, but no discharge from the nose, and no rash. He was feverish for a few days, and complained of pains in the loins towards the end. His water was scanty and high coloured. John had a very bad sore throat, which had evidently ulcerated. He had a red rash all over him. There was no diarrhœa, and the desquamation was still going on when I first called.

The last to be attacked was the little one whom I attended. She had been "shivering and feverish," the throat was congested, the tongue of typical "strawberry" appearance, marked rash, high fever, delirium, enlarged glands and desquamation.

I enquired most carefully into every possible source of infection, but was utterly unable to find any. They had seen no strangers, had had nothing by carrier, and had received no letters, save what I could trace clearly for very many months past. The house itself stands right away from any road, and is very unlikely to lend any colour to the tramp theory.

Now, how can we explain these cases? Those who are anxious to prove the specific infection theory proceed to do so by begging the question, and saying either that it could not be Scarlet Fever, because there was no infection; or that there must have been some infection which we failed to trace, because there was Scarlet Fever. Then we can have a far fetched hypothesis about latent Scarlet Fever, though I was unable to discover that any such disease had ever been in the house at all. Then there is infection by air.

And finally, there is the theory of Progressive Evolution, which I now bring forward, not, I hope, without strong evidence in its behalf. By the energy with which some 'specialists' oppose the introduction of the principle of evolution in disease, one would suppose that there existed some enormous *à priori* improbability against it. Whereas, it appears to me, and I have endeavoured to shew, that all the weight of *à priori* evidence tends to prove that in disease, as in nature generally, evolution results in the origin of species.

What could be more like the **progressive development** of specific Scarlet Fever from a sore throat due to indifferent germs that gradually acquired specific infectiveness? I would further point out that the time that elapsed between the entrance of the organisms into the lad (allowing about five days for incubation), and the final development of typical Scarlet Fever in the girl was about 26 days, or nearly a month. Now if, as has been stated, one thousand generations of bacteria come into existence in a fortnight, some 1,700 generations must have lived their life in the meanwhile. Surely there was time enough here, for the constant operation of similar surroundings, to impress new characters and to call forth new phases of functional activity!

The principal objections to this theory appear to me to arise, as is the case in respect of most innovations, from the difficulty of shaking off old ideas, especially when those ideas are more or less forcibly perpetuated by terms in common use. The term, "specific disease," is so widely prevalent, that it probably scarcely enters into any one's head to enquire to what extent it is accurate. The term itself arose from the clinical characters of these diseases, though it has doubtless received a fresh importance from the orthodox view of the Germ Theory. Now, in so far as it is taken to mean that there are certain diseases which tend to reproduce their like with more or less of similarity, and that a direct parentage may frequently be traced for their existence in individual cases, there can be no objection to its use. But where it is also taken to negative the converse, and to imply that like cannot transmit unlike by a process of 'variation' under any circumstances, and that one case inevitably proves infection from a pre-existent one, then it appears to me that the weight of clinical evidence is rapidly accumulating against it.

For it must not be forgotten that positive evidence, however numerically weak, is always of more force than negative evidence, however numerically strong. But even as to numerical strength, I think it only wants attention calling to anomalies, and an accurate record of them as departures from the type, instead of a forcible conformation of them to the type, to shew how very large a class these anomalies form.

In ZIEGLER'S Pathological Anatomy, translated by Dr. MACALISTER, from which I have already quoted, considerable attention is paid to the biology of bacteria. Their conditions of life, the requisite temperature of the nutrient medium, their influence on the nutrient liquids, their relation to fermentations and to disease, are all fully discussed. Yet after this exhaustive consideration of their conditions of existence, and an admission that the characters of the organisms are greatly changed or modified by their surroundings, and even after the admission that under certain conditions non-pathogenous bacteria may possibly become pathogenous, we read on page 290, "The infective diseases form a group distinguished by their marked specific character, and their special mode of origin. The specific character is manifested in this—that the disease runs a similar course in each individual attacked, conditioned solely by the nature of the morbific virus (?). As to the genesis of the disease, it is always referable to the passage into the organism of a poison from without." This assertion of a state of things so contrary to what we should logically expect, after a consideration of what we have been told in the same work of the effect of the environment upon such organisms is explained by the following passage (page 301, Art. 208) :—

"If we accept for a moment the hypothesis that all or most infective diseases (other than those due to animal parasites) are caused by the development of bacteria in some tissue or fluid of the body, we are met at once by the question whether in that case each specific form of disease has a corresponding specific bacterium. From a clinical standpoint, this question must be answered in the affirmative. The most marked feature of the infective diseases is just this, that they run a typical and special course. Even though this may in individual cases be modified by various influences, it is in general so characteristic, so pathognomic, that the disease can often be diagnosed by its course alone.

We should, therefore, have no hesitation in inferring from the specific course of the disease, that the virus which excites it is also specific."

Now, I cannot but regard this as a marked instance of the effect of mental bias. Failing completely to recognise the force of the fact that the opposition still largely entertained by many to the Germ Theory, is due to the rigid specific view being incompatible with certain clinical irregularities, and, after admitting every point required as regards the organism itself, and the probability of its alteration of function under change of environment, to overcome those objections, the Author proceeds to argue that however theoretically possible, and even to an evolutionist probable, such change is not an actual fact because the absence of clinical irregularities, and the constant clinical specificity of disease, implies a constant functional specificity of the disease germ.

It has been my endeavour to shew that this clinical specificity of disease is only partially true; and, indeed, the very existence of such numerous exceptions as are from time to time recorded, constitutes the chief support of the opponents of the Germ Theory.

Were I dealing with universal specificists to whom the light of the great doctrine of evolution had not yet penetrated, I might cease to wonder that a bias determining to the preservation of the specific theory, should unconsciously blind the advocate's eyes to clinical evidence which would imperil that dogma. But dealing, as I must be, with those to whom the cardinal truth of evolution is the basis of all scientific knowledge, I cannot but wonder that they should shrink from applying that doctrine in a case where, as it would appear even to themselves, it is pre-eminently applicable, because the traditions of a time ere it was understood have blinded the eyes to a great mass of facts, and engendered a habit of thought which leads them, in the words of Dr. HUGHLINGS JACKSON, already quoted, "to study cases as the symptoms shew approaches to certain clinical standards, and not as they shew departures from healthy types."

In conclusion, I would summarise the evolution and life history of Disease Germs in the following

CYCLE:

(1) A microorganism thrives and multiplies at the expense of the medium in which it lives, splitting up the proximate elements thereof, retaining some for food, freeing others, and thus producing an analytical change in the composition of the medium; finally returning thereto "unorganised ferments," "organic alkaloids," "colouring matters," and other excreta which are the result of its own vital processes, thus inducing also a synthetical change. These

changes in the chemical composition of the medium inevitably originate a series of phenomena, which in dead media are termed putrefaction or fermentation, in living media disease. In the latter case the alteration in the media produces also reactionary physiological phenomena, complicating those of a physico-chemical nature.

(2) On transference of this organism from its natural to a foreign medium, a "struggle for existence" ensues. Should the organism fail to succeed in 'adjusting itself to its environment' it will die out: on the contrary, should it succeed in so doing, it will be by adapting itself to some other kind of nutriment, its choice being probably influenced by one or both of two circumstances affecting the pabulum, viz.: (1) abundance of any available kind; (2) greater or less similarity to the normal pabulum. The nature of the food selected, with the nature of the excreta dependent thereon, will determine the character of the changes induced in the medium, and consequently of the resultant phenomena, whether merely physico-chemical, or also in addition physiological.

(3) The multiplication of generations which proceeds very rapidly, will modify the original habits of the race, and originate an hereditary tendency to select, wherever it is forthcoming, a similar pabulum. This will entail similar excreta, and in a similar medium a totality of similar changes

resulting in similar phenomena; while variations in both changes and phenomena more or less marked will ensue, in proportion to the difference between the media.

(4) Should a retransference into a very heterogeneous medium take place, the cycle will begin again at (1.) *

Here then, we have an explanation of all the phenomena attendant upon the specific infectious diseases. Their **origin** is thus explained, their **specificity** is accounted for by successive cultivation in similar constitutions. **Variability in transmission** and **Nondescript** cases merely mark a struggle for existence, ending in the survival of the fittest which has had vitality enough to adjust itself to its new environment. **Immunity,** whether hereditary or acquired, may be supposed to consist in an alteration in the character of the ultimate tissue cells, consequent on their supremacy in the struggle with the bacterian invaders for oxygen and other needful materials; while **indefinite incubation** merely points to variations in the intensity and protractedness of the struggle between the germs and the tissue cells. **De Novo origin** is no longer a bugbear, for it merely means evolution of function, as the result of a process of Natural Selection, and does not imply heterogenesis; and finally, while Evolution is shewn to reign here as in Nature everywhere, supreme, the Specific Diseases no longer mar the universal truth, that Pathology is "Physiology gone wrong," and that disease, instead of being in itself an entity, is a mere departure from the normal line of health!

---

* "This speculation, when applied to the case of other diseases in which bacteria are suspected, acquires a very striking significance. It offers an explanation, which we do not find in the theory of fixity of specific physiological activities of bacteria, of (1) the importance of organic refuse in "breeding" disease; (2) of the apparently spontaneous outbreak of zymotic disease (really the acquirement of parasitic adaptation by species of bacteria present generally in a harmless state); (3) of the paradox that, according to the 'fixity' theory, millions of these pathogenic bacteria must everywhere be diffused in a state ready for active infection, and yet men and animals are not habitually attacked by them, but only at intervals of time and place, so as to cause what are known as 'epidemics.'"—"Bacteria and Disease:" Leading Article, *Medical Times and Gazette*, Nov. 3, 1883.

APPENDIX.

In this Appendix I had hoped to present some results which would be of service in elucidating more fully the question I have been considering.

To that end, I sent out in August and September last, 1,500 circulars as follows :—

North Lodge,
Kineton, Warwick,
September 1, 1883.

Dear Sir,

The following conclusions were drawn and suggestions thrown out at the close of a paper on the "Ætiology of the Acute Specific Infectious Diseases," read last year in the Section of Public Medicine at the British Medical Association's Meeting, and published in the *Journal* of September 30th :—

> "1. I conceive the processes of **development and the origin of species** to be still going on in diseases.
>
> 2. It is hypothecated that **organisms not essentially disease-germs, may become so** on introduction within the human economy; that the form taken by the **disease depends upon the pabulum** selected by the germ, and that **specific characters are in course of time developed** in the germs.
>
> 3. This process accounts for the **de novo origin** of the so-called acute specific infectious diseases; for **abnormal cases** of these diseases; and for **changes of type** in transmission.
>
> 4. This process is probably **evolving, through the many unclassifiable and abnormal cases, fresh 'specific' diseases** in the future.
>
> In conclusion, if I may without presumption venture to indicate a few lines of research, I would say :—
>
> 1. It is to **abnormal rather than to normal cases** that I would look for the future settlement of the question.
>
> 2. It is desirable that in cases of infectious disease, where no origin can be traced, special attention should be paid to any **irregularities indicating imperfect or progressive development.**

3. In cases of seeming **change of type,** special inquiry should be made into **hereditary predisposition.**

4. Further chemical, microscopical, and spectroscopical **examinations of the blood** should be made in fever cases; especially comparing the results in cases respectively of typical and abnormal characters.

5, Finally, special attention should be paid to **cases and epidemics of such affections as form prominent symptoms** in certain well-established diseases: *e.g.*, infectious sore throat, and the question of its development into scarlatina or diphtheria; catarrhal diarrhœa, and its relation to typhoid fever; and the relation of rotheln, which is undoubtedly autogenetic, to measles and scarlatina which are commonly held to be specific."

As I am desirous of following this investigation yet further, and of obtaining more evidence upon three classes of anomalies (viz.: Variability in Transmission, Hybrids, and Diversity from Unity), as well as upon the question of the *de novo* origin of certain "Acute Specific Infectious" Diseases, I venture to ask if you will do me the kindness to reply to the questions on the next page, and to transmit this circular *unsealed* by post to me.

If you will so far favour me, I shall feel grateful for this act of professional fellowship.

I am,
Yours faithfully,
KENNETH W. MILLICAN,
B.A. Cantab., M.R.C.S.

Only 58 gentlemen favoured me with replies to the circular, and to these I must add a courteous note from Sir JAMES PAGET, stating that he had long ceased to come in contact with the diseases enquired about; and an equally courteous letter from Dr. THORNE THORNE, referring me to certain of his published views, which I have perused with great interest, some of which I have embodied in my Essay, and most of which, I think, tend very strongly to bear out the views I advocate.

Had I received, as I had hoped, some three or four hundred replies to my circular, it was my intention to publish those replies *in extenso* in this Appendix. The replies received, however, are so few in number, that I refrain from publishing them at present; and merely subjoining an analysis of the answers received, I look forward to a future occasion when the re-issue of my circular will call forth a sufficient number of replies to make their verbatim publication a work of considerable interest. And here I must tender my sincere thanks to those gentlemen whose names I append, who have done me the kindness to reply more or less fully, so far as they were able, to the questions which I ventured to submit to them.

I think the analysis will sufficiently shew, so far as it goes, that as I stated in the body of the work, those anomalies and irregularities of origin which give colour to the views I promulgate, are more common than is usually imagined by those who have not paid special attention to the subject, and I look forward with confidence to the result of a more extended enquiry, embracing a greater field for observation.

The following are the names of the gentlemen who have kindly aided me in this enquiry :—

ALLEN, R. M., Esq., F.R.C.S., Dep. Surg. Gen., Grantham.
ANDERSON, E. C., Esq., M.A., M.D., Darlington.
BARNES, E. C., Esq., M.R.C.S., Hammersmith.
BATELY, R. G., Esq., Gorleston.
BEYNALL, S. F.. Esq., Runcorn.
BOWKETT, T. E., Esq., M.R.C.S., London.
BRAIDWOOD, P. M., Esq., M.D., Birkenhead.
BROOMHEAD, C., Esq., M.D., Oldham.
CHAPMAN, G., Esq., M.R.C.S., Yoxall.
CHEADLE, W. B., Esq., M.A., M.D., F.R.C.P., London.
CHILD, E., Esq., M.R.C.S., L.S.A., New Malden.
CLARKE, A. B., Esq., L.R.C.S., L.S.A., Chatteris.
CLARKE, F. W., Esq., M.R.C.S., Bury St. Edmunds.
CLARKE, R. A., Esq., M.R.C.S., Bolton-le-Moors.
COLLINS, W. J., Esq., M.D., B.Sc., B.S. Lond., London.
COOMBS, C. P., Esq., M.D., Castle Cary.
DOLAN, T. M., Esq., F.R.C.S. Ed., Halifax.
DONOVAN, W., Esq., L.R.C.P., Whitwick.
DOUGALL, J., Esq., M.D., F.R.C.P., Glasgow.
FAGGE, C., Esq., M.R.C.S., Hythe.
FOSTER, P., Esq., M.D., Leeds.
FYFE, G., Esq., M.D., Gainsborough.
GREENE, J., Esq., M.R.C.S., Birmingham.

Hatherley, H. R., Esq., M.R.C.S., Nottingham.
Haynes, F. H., Esq., M.D., M.R.C.P., Leamington.
Haynes, Stanley, Esq., M.D., Malvern.
Harley, G., Esq., M.D., F.R.S., London.
Johnson, S., Esq., M.D., Stoke-upon-Trent.
Jones, A. T. Lloyd, Esq., L.R.C.P., Hadley.
Knox, J., Esq., M.D., Bakewell.
Larke, R.D., Esq., M.R.C.S., Cirencester.
Low, R. B., Esq., M.D., Helmsley.
Macnab, J., Esq., M.D., F.R.C.S.E., Abbe Stornoway, N.B.
Makuna, M. D., Esq., M.R.C.S., L.R.C.P., London.
Martin, J. W., Esq., M.D., Sheffield.
May, A. E., Esq., M.R.C.S., L.R.C.P., Moreton Hampstead.
Meredith, J., Esq., M.D., Wellington.
Mockett, G. T., Esq., M.R.C.S., Tyldesley.
Neale, R., Esq., M.D., London.
Newth, A. H., Esq., M.D., Hayward's Heath.
Owen, E. B., Esq., F.R.C.S., London.
Parsons, D. W., Esq., L.R.C.P., Liverpool.
Pearse, W. H., Esq., M.D., Plymouth.
Pike, J. B., Esq., M.R.C.S., Loughboro'.
Phillips, G. A., Esq., M.R.C.S., Walsall.
Robinson, A. H., Esq., M.D., Hull.
Shea, J., Esq., M.D., Reading.
Squire, W., Esq., M.D., F.R.C.P., London.
Strange. W., Esq., M.D., Worcester.
Sunderland, W., Esq., M.R.C.S., Thaxted.
Sykes, J. F. J., Esq., M.B., B.Sc., London.
Sykes, W., Esq., M.R.C.S., Rotherham.
Thomson, W. Sinclair, Esq., M.D., London.
Thursfield, T. W., Esq., M.D., M.R.C.P., Leamington.
Tibbits, J., Esq., M.D., Warwick.
Wallace, A. W., Esq., M.D., Parsonstown.
Wilson, G., Esq., M.A., M.D., F.R.S.E., Leamington.
Wood, R., Esq., M.D., Bromsgrove.

The analysis of the replies is as follows :—

QUESTIONS.	ANSWERS.
Variability in Transmission. 1.—Have you ever seen a case or cases where one kind of "specific" disease appears to have transmitted another disease, *e.g.*, scarlatina giving rise to measles?	Yes .. .. 17 No .. .. 32 Doubtful .. .. 5 No Answer .. .. 4 — 58
*** Hybrids.** 2.—Have you ever seen cases of "hybrid" disease, *i.e.*, of a character so indefinite that it would be difficult to class them ; or though being in most points characteristic, yet presenting features of an anomalous kind, *e.g.*, typhoid fever with a scarlatina eruption, or the like?	Yes .. .. 35 No .. .. 13 Doubtful .. .. 7 No Answer .. .. 3 — 58
Diversity from Unity. 3.—Have you ever seen a case where the same disease poison, or the same combination of insanitary conditions, appears to have originated in different persons diseases of different types, *e.g.*, one source, whether infectious or insanitary, originating typhoid in one patient, scarlatina in another?	Yes .. .. 25 No .. .. 16 Doubtful .. .. 6 No Answer .. .. 11 — 58
De Novo Origin. 4.—Have you ever seen, and how often, cases were careful investigations lead to the conclusion of a *de novo* origin for—	Small Pox .. .. 1 Typhus .. .. 13 1 doubtful. Typhoid .. .. 24 3 doubtful. Diphtheria .. 24 3 doubtful. Scarlet Fever .. 13 8 doubtful. Measles .. .. 7 5 doubtful. Rotheln .. .. 4 4 doubtful. Whooping Cough .. 9 7 doubtful. Varicella .. .. 1 Cholera 1
Progressive Development. 5.—Have you ever seen a series of cases commencing with a prominent symptom of some recognised "zymotic," and progressing through various patients to a well marked typical affection ; *e.g.*, first case, diarrhœa, developing in later cases into typhoid fever ; or, first case, sore throat, developing in successive cases into diphtheria or scarlet fever?	Yes .. .. .. 24 No .. .. .. 11 Doubtful .. .. 11 No Answer .. .. 12 — 58

* As remarked above, the choice of this word was unfortunate. "Nondescript" would more correctly convey my meaning.

Tabulated List of recorded anomalies in the acute specific infectious diseases, arranged according to the classification adopted in the body of the work. *

CLASS I.—Variability in transmission. Cases where one kind of disease appears to have given rise to disease of another kind.

NAME OF OBSERVER.	NATURE OF CASE AND REFERENCE.
MEREDITH, Dr. JOHN.	Diphtheria giving rise to Scarlatina.—*Lancet*, May 26, 1883.
BEYNALL, S. F., Esq.	Has seen Scarlatina apparently give rise to Measles.—*Reply to my Circular.*
BROOMHEAD, Dr. C.	Measles apparently the result of Vaccination.—*Reply to my Circular.*
COLLINS, Dr. W. J.	Has seen two cases where apparently Scarlatina gave rise to Diphtheria.—*Reply to my Circular.*
DOUGALL, Dr. JOHN.	Apparent transmission of Scarlatina from Measles.—*Reply to my Circular.*
LOW, Dr. R. BRUCE.	Apparent transmission of Diphtheria from "pure Scarlet Fever."—*Reply to my Circular.*
MEREDITH, Dr. JOHN.	Diphtheria apparently giving rise to Mumps.—*Reply to my Circular.*
PEARSE, Dr. W. H.	Scarlatina apparently evolved from Measles—six weeks at sea.—*Reply to my Circular.*

---

* This list, I regret to say, is very incomplete. I hope hereafter, in a further communication, to render it much more exhaustive.

NAME OF OBSERVER.	NATURE OF CASE AND REFERENCE.
PIKE, J. B., Esq.	Apparent transmission of Scarlatina by Diphtheria, and *vice versa.—Reply to my Circular.*
THOMSON, Dr. W. S.	Scarlatina apparently originating Diphtheria; also Typhoid originating Diphtheria.—*Reply to my Circular.*
WILSON, Dr. GEO.	Scarlatina inducing Puerperal Fever.—*Reply to my Circular.*
THURSFIELD, Dr. T. W.	Apparent transmission of Diphtheria by Scarlet Fever.—*Reply to my Circular.*
DOYLE, Dr.	Observed at the Medical Society of the College of Physicians in Ireland, that "milk has been recognised as a means of conveying this disease. Within the last two months three cases of Typhoid Fever had been under his care, and it was found that one of the men who milked the cows from which milk was supplied was suffering from syphilis. The question of the possible connection between diseases of the genital organs and Typhoid Fever should be investigated." [Rather a forced connection. K. W. M.]—*Brit. Med. Journ.*, March 5, 1883.
PEARSE, Dr. T. F.	"Sore throat" transmitting Croup and Diphtheria—[But doubtful. K. W. M.]—*British Medical Journal*, Oct. 8, 1881.
DOWNES, Dr. ARTHUR.	"Spreading Quinsey" and Diphtheria (Questionable whether they are not modifications of each other. K. W. M.)—*British Medical Journal*, Dec. 21, 1878.
BRITISH MEDICAL JOURNAL LEADER.	Operations giving rise to infective Scarlatina—three cases recorded.—Nov. 9, 1878.

CLASS II.—Nondescripts. Cases of a character so indefinite that it would be difficult to class them; or which, being in most points characteristic, yet present features of an anomalous kind.

Name of Observer.	Nature of Case and Reference.
Beevor, Dr. W. C.	Measles rash in Typhoid Fever, appeared on third day on feet and legs, extending to chest and upper extremities.—*Brit. Med. Journ.*, May 19, 1883.
Whipham, Dr.	Scarlatina rash in Typhoid. Sore throat, bright erythematous eruption on trunk and arms, right tonsil much swollen, temp. 105° F. No Typhoid eruption. P.M. ulceration of Peyer's patches.
	Child had Scarlatina, Measles, and Whooping Cough. Fever first shewed day before admission; temp. on admission, 104.2° F.; Scarlet Fever tongue, Scarlet eruption on legs on *second day* of fever, tonsils red and swollen: this was Oct. 6. On Oct. 17, charcteristic Enteric Fever spots; from this date symptoms those of Enteric Fever. P.M. Ulceration of Peyer's patches.
Harley, Dr. John.	Gives 28 cases of Scarlatina accompanied by swelling of Peyer's patches, but no ulceration. Also three cases of "mixed" Enteric and Scarlet Fevers.
Whipham, Dr.	Had recently seen Variola preceded by abdominal Erythema.—*Brit. Med. Journ.*, April 21, 1883.
	A child, three years, was vaccinated, and developed well-marked Chicken Pox, followed by a Scarlatinal eruption.—*Brit. Med. Journ.*, Jan. 6, 1872.

Name of Observer.	Nature of Case and Reference.
Freer, F. A., Esq.	After relating a case of concurrence of Measles with Variola, and subsequently of Scarlatina with Variola (the latter caught from the first patient), says:—"Dr. Broadbent, of South Hetton, has just related to me a case which occurred in his practice a few weeks ago, similar to that of Jaue M. (the second.) The desquamation was perfectly established when the eruption had become pustular. *There was not "another case of Scarlatina in the district at the time*."—*Brit. Med. Journ.*, April 13, 1872.
Woods, Dr. C. R.	Typhoid Fever assuming an *intermittent* type. (It is, however, to be noticed that the patient had had Intermittent fever, and his constitution was therefore probably still infected with malarial poison. K.W.M.)—*Lancet*, May 20th, 1882.
Hallopeau, M.	Scarlatinal rash and desquamation in Acute Rheumatism. Second attack of the kind (shewing blood predisposition. K.W.M.) *Journ. Med. de Paris*, Nov. 4, 1882.
Barnes, E. C., Esq.	I had a case of Typhus with Scarlet Fever rash and sore throat (adult), the rash was obvious five days after to two consultants. I saw three typhus children in the same outbreak, with Scarlet Fever rash of face and neck."—*Reply to my Circular.*
Beynall, S. F., Esq.	Has seen Typhoid with Scarlatinal rash.—*Reply to my Circular.*
Collins, Dr. W. J.	Variola with Scarlatinal rash. Hybrid between Scarlatiua and Diphtheria. Typhoid with *typical* Rötheln rash. Puerperal Fever closely resembling Typhoid.—*Reply to my Circular.*

NAME OF OBSERVER.	NATURE OF CASE AND REFERENCE.
BROOMHEAD, Dr. C.	Typhoid Fever with abundant Scarlatinal rash. *Reply to my Circular.*
JONES, A. T. LLOYD, Esq.	Scarlatinal eruption in Typhoid.—*Reply to my Circular.*
MAKUNA, M. D., Esq.	Apparent hybrids between Measles and Scarlatina—between Scarlatina and Diphtheria.—*Reply to my Circular.*
MARTIN, Dr. J. W.	Scarlatina rash in Typhoid.—*Reply to my Circular.*
THURSFIELD, Dr. T. W.	I have seen a case, the diagnosis in which has been doubtful as between Typhoid, Acute Rheumatism, Chorea, and Blood poisoning. —*Reply to my Circular.*
PEARSE, Dr. W. H.	Case of Typical Measles on Ship *Oasis*, 19 days after sailing. This was followed by two cases of febrile disease with *Vesicular Eruption.* Measles? A subsequent case of Measles.—*Med. Times and Gaz.*, Sept., 1881.
BUSHE, Dr. CECIL.	Enteric Fever with Typho-malarial symptoms. —*Brit. Med. Journ.*, April 30, 1881, p. 686.
LADELL, Dr. W. J. S.	Cases of Rötheln.—The face, neck, and arms were covered with a rash resembling that of Scarlatina; while the abdomen, chest, and legs were plentifully sprinkled with the larger patches peculiar to Measles. Five months afterwards one patient suffered from Measles and another from Scarlatina.—*Brit. Med. Journ.*, July 30th, 1881.

NAME OF OBSERVER.	NATURE OF CASE AND REFERENCE.
DUKES, Dr. C.	Outbreak of Infectious Sore Throat, in which the symptoms were analagous to Influenza, with the exception that instead of Catarrh of the eyes and nose, we had Catarrhal Sore Throat. All the other symptoms were absolutely indentical with our usual Epidemic Influenza.—*Brit. Med. Journ.*, Sept. 24th, 1881.
SNOW, Dr. H. L.	Case of Spurious Diphtheria.—*Brit. Med. Journ.*, Oct. 22nd, 1881.
MAY, GEO., JUN., Esq.	Scarlatina rash and Sore Throat in Septicæmia (non-infectious.)—*Brit. Med. Journ.*, Dec. 21, 1878. (But see Mr. Howse's cases for Infection. K.W.M.)

CLASS III.—Diversity from Unity. Cases where, from a common source, two or more specific diseases appear to take their origin.

NAME OF OBSERVER.	NATURE OF CASE AND REFERENCE.
	Pollution of well water with infiltrated sewage gave rise in two over-crowded cottages at West Newton to (*a*) Typhoid (simple), (*b*) Typhoidal Pneumonia, (*c*) Febrile disease with pseudo scarlatinal eruption.—*British Medical Journal*, Jan. 6, 1872.
BOWKETT, T. E., Esq.	Has known cases of same combination of insanitary conditions give rise to Typhoid, Scarlet Fever, and Diphtheria (without infection? K. W. M.)—*Reply to my Circular.*

Name of Observer.	Nature of Case and Reference.
Meredith, Dr.	Diphtheria and "illness of a blood poisoning character," in the same house, from same insanitary conditions. Diphtheria and Mumps, apparently arising from the same insanitary condition. Sanitary Report, *Wellington Weekly News*, Feb. 1, 1883.
Anderson, Dr. E. C.	During and after the emptying of a middenstead the husband developed carbuncle, the wife inflammation (erysipelas ?) of the throat and neck, with tonsillitis; and the daughter with cyanche tonsillaris.—*Reply to my Circular.*
Bately, R. G., Esq.	T. C., aged about 28, suffered from Diphtheria (due to insanitary conditions of house), and during his illness one child died from croup, another had an attack of Scarlatina and recovered.—*Reply to my Circular.*
Beynall, S. F., Esq.	Has seen Erysipelas, Measles, and Scarlatina, in same house, apparently arising from same insanitary conditions.—*Reply to my Circular.*
Collins, Dr. W. J.	Has seen Diphtheria, Scarlatina, and Measles, arise from (so far as could be ascertained) the same combination of insanitary conditions.—*Reply to my Circular.*
Makuna, M. D., Esq.	Cholera, Famine, Spirillum, Relapsing and Typhus Fevers, from similar insanitary conditions, in India.—*Reply to my Circular.*
Martin, Dr. J. W.	Typhoid and sore throat, in same house, from same insanitary conditions.—*Reply to my Circular.*

NAME OF OBSERVER.	NATURE OF CASE AND REFERENCE.
MADDEN-MEDLICOTT, Dr. C. W. C.	Gives an account of an epidemic of severe facial Erysipelas, mingled with cases of Typhoid Fever, which occurred in the Somerset and Bath Lunatic Asylum in 1879-80. (There can be no reasonable doubt that, as Dr. Medlicott appears to think, the common cause lay in insanitary conditions of drainage, &c., to which he refers in detail. K. W. M.)—*British Medical Journal*, Oct. 29, 1881.
MOTT, F. W., Esq.	Typhoid, complicated with Diphtheria.—*British Medical Journal*, July 30, 1881.—(This case is a doubtful one, as the Diphtheria may have been contracted separately.
NEALE, Dr. R.	Has often seen Measles, Scarlatina, Rötheln, and Diphtheria, apparently arising (in different combinations) from the same cause.—*Reply to my Circular.*
OWEN, E. B., Esq.	True Croup and Diphtheria, apparently from the same source, or the latter from the former.—*Reply to my Circular.*
STRANGE, Dr. W.	Diphtheria and Typhoid.—*Reply to my Circular.*
PALMER, H. C., Esq.	Typical Typhoid Fever,—after seven days, Diphtheria. "Three houses in the same row had Typhoid Fever; in none of these were diphtheritic symptoms present."—*Lancet*, Aug. 18, 1883, p. 301.

The following letter appeared in the *Lancet*, of May 14th, 1881, p. 812:—

"SPECIFICITY AND EVOLUTION IN DISEASE."

"To the Editor of the *Lancet*."

"Sir,—Your editorial comments on 'Blood Diseases and the Germ "Theory' have induced me to send you the following reflections on the "collateral question as to how far our commonly accepted notions of "specificity of diseases should be modified by the doctrine of evolution.

"This doctrine, which has so rapidly and entirely altered the face of "the organic world, as we conceive it, has as yet made but a feeble im-"pression, if any, in Medical Science and Pathology.

"The importance of the principle of specificity in disease has been "clearly and generally recognised since the time of LANNAEC and "BRETONNEAU; TROUSSEAU in one of his clinics devoted to the sub-"ject, remarks of specificity, '*Elle domine toute la pathologie, toute la* "*thérapeutique, en un mot toute la Médecine.*' And the same belief, "tacit or avowed, would seem to pervade all modern medical literature.

"Now, the argument upon which medical writers base their theory "of specificity, consists in an appeal to the analogy which exists between "the natural history of diseases and the natural history of animals and "plants. In the words of SYDENHAM, '*Unaquœque morborum non* "*minus quam animalium aut vegetabilium species affectiones sibi proprias* "*perpetuas ac pariter univocas ab essentia sua promanantes sortita est*;' "and until the last few years no better argument was possible.

"Since, however, as regards the organic world, we have come to "speak of an origin of species, and learnt to trace their differentiation "under the influence of evolution to the eventual survival of the fittest, "I would ask, should not our views concerning specificity of diseases "undergo a similar modification? Is not the theory of specificity "swallowed up in the larger theory of evolution? As with animals, so "with diseases; may we not henceforth regard the several species and "genera as descendants from a common ancestor? Starting from a new "specific inflammation or fever, may there not arise therefrom, under "favourable conditions, an inflammation or a fever different from the "parent stock, and capable of reproducing itself? I think so. Look "at tubercle, a disease characterised by such well-marked features as to "have been long since classed as a specific disorder, but whose natural "history, revealed by the experiments of COHNHEIM, BURDON-SANDERSON, and others, shows that it may be derived by cultivation from "the simplest common inflammation, as that resulting from a chemical "irritant; the non-specific has developed specific characters, the "homogeneous become the heterogeneous. So it is with other diseases; "even the ordinary stages of a common inflammation, pimple, vesicle, "pustule or scab, foreshadow and typify diseases which we call specific, "wherein one or other of the stages predominate, while the rest are "subordinate. The common ancestry of specific diseases, once recognised, would do much to remove the hard and fast line so often drawn "between disease and disease in text books and dissertations of which "nature knows nothing."

"I am, Sir,

"Yours obediently,

"W. J. COLLINS, B.Sc., M.R.C.S.

"*St. Bartholomew's Hospital,*
"*April 23rd, 1881.*"

136 Gower Street,
October, 1883.

A

CATALOGUE OF WORKS

PUBLISHED BY

H. K. LEWIS

LONDON
H. K. LEWIS, 136 GOWER STREET, W.C.
1883.

CATALOGUE OF WORKS

Published by

H. K. LEWIS, 136 GOWER STREET,

LONDON, W.C.

G. GRANVILLE BANTOCK, M.D., F.R.C.S., EDIN.

Surgeon to the Samaritan Free Hospital for Women and Children.

A PLEA FOR EARLY OVARIOTOMY. Demy 8vo, 2s.

FANCOURT BARNES, M.D., M.R.C.P.

Physician to the Chelsea Hospital; Assistant Obstetric Physician to the Great Northern Hospital, &c.

A GERMAN-ENGLISH DICTIONARY OF WORDS AND TERMS USED IN MEDICINE AND ITS COGNATE SCIENCES. Square 12mo, Roxburgh binding, 9s.

ROBERTS BARTHOLOW, M.A., M.D., LL.D.

Professor of Materia Medica and Therapeutics, in the Jefferson Medical College of Philadelphia, etc.

I.

A TREATISE ON THE PRACTICE OF MEDICINE FOR THE USE OF STUDENTS AND PRACTITIONERS. With Illustrations, 2nd Edition, large 8vo, 21s.

II.

A PRACTICAL TREATISE ON MATERIA MEDICA AND THERAPEUTICS. Fourth Edition, Revised and Enlarged, 8vo, 16s.

GEO. M. BEARD, A.M., M.D.

Fellow of the New York Academy of Medicine.

AND

A. D. ROCKWELL, A.M., M.D.

Fellow of the New York Academy of Medicine.

A PRACTICAL TREATISE ON THE MEDICAL AND SURGICAL USES OF ELECTRICITY; including Localized and General Faradization; Localized and Central Galvanization; Electrolysis and Galvano-Cautery. Third Edition, With nearly 200 Illustrations, roy. 8vo, 28s.

A. HUGHES BENNETT, M.D.

Member of the Royal College of Physicians of London; Physician to the Hospital for Epilepsy and Paralysis, Regent's Park; and Assistant Physician to the Westminster Hospital.

I.

A PRACTICAL TREATISE ON ELECTRO-DIAGNOSIS IN DISEASES OF THE NERVOUS SYSTEM. With Illustrations, 8vo, 8s. 6d.

II.

ILLUSTRATIONS OF THE SUPERFICIAL NERVES AND MUSCLES, WITH THEIR MOTOR POINTS, A knowledge of which is essential in the art of Electro-Diagnosis. (Extracted from the above). 8vo, paper cover 1s. 6d., cloth 2s.

DR. THEODOR BILLROTH.

Professor of Surgery in Vienna.

GENERAL SURGICAL PATHOLOGY AND THERAPEUTICS. In Fifty-one Lectures. A Text-book for Students and Physicians. Translated from the Fourth German Edition with the special permission of the Author, and revised from the Tenth German Edition, by C. E. HACKLEY, A.M., M.D. Copiously illustrated, 8vo, 18s.

G. H. BRANDT, M.D.

I.

ROYAT (LES BAINS) IN AUVERGNE, ITS MINERAL WATERS AND CLIMATE. With Preface by DR. BURNEY YEO. Second Edition, Frontispiece and Map, crown 8vo, 2s 6d.

II.

HAMMAM R'IRHA, ALGIERS. A Winter Health Resort and Mineral Water Cure Combined. With Frontispiece and Map, crown 8vo, 2s. 6d.

GURDON BUCK, M.D.

CONTRIBUTIONS TO REPARATIVE SURGERY; shewing its application to the Treatment of Deformities, produced by Destructive Disease or Injury; Congenital Defects from Arrest or Excess of Development; and Cicatricial Contractions from Burns. Illustrated by numerous Engravings, large 8vo, 9s.

FREEMAN J. BUMSTEAD, M.D., LL.D.

Late Professor of Venereal Diseases at the College of Physicians and Surgeons, New York.

THE PATHOLOGY AND TREATMENT OF VENEREAL DISEASES. Fourth Edition, revised, enlarged, and in great part re-written by the author, and by ROBERT W. TAYLOR, A.M., M.D. With 138 woodcuts, 8vo, 25s.

ALFRED H. CARTER, M.D. LOND.

Member of the Royal College of Physicians; Physician to the Queen's Hospital, Birmingham, &c.

ELEMENTS OF PRACTICAL MEDICINE. Second Edition, crown 8vo, 9s. [*Just ready.*

P. CAZEAUX.

Adjunct Professor in the Faculty of Medicine of Paris, &c.

A THEORETICAL AND PRACTICAL TREATISE ON MIDWIFERY; including the Diseases of Pregnancy and Parturition. Revised and Annotated by S. TARNIER. Translated from the Seventh French Edition by W. R. BULLOCK, M.D. Royal 8vo, 175 Illustrations, 30s.

JOHN COCKLE, M.A., M.D.

Physician to the Royal Free Hospital.

ON INTRA-THORACIC CANCER. 8vo, 4s 6d.

W. H. CORFIELD, M.A., M.D. OXON.

Professor of Hygiene and Public Health in University College, London.

DWELLING HOUSES: their Sanitary Construction and Arrangements. With 16 pages of Illustrations, crown 8vo, 3s 6d.

J. THOMPSON DICKSON, M.A., M.B. CANTAB.

Late Lecturer on Mental Diseases at Guy's Hospital.

THE SCIENCE AND PRACTICE OF MEDICINE IN RELATION TO MIND, the Pathology of the Nerve Centres, and the Jurisprudence of Insanity; being a Course of Lectures delivered at Guy's Hospital. Illustrated by Chromo-lithographic Drawings and Physiological Portraits. 8vo, 14s.

HORACE DOBELL, M.D.

Consulting Physician to the Royal Hospital for Diseases of the Chest, &c.

I.

ON DIET AND REGIMEN IN SICKNESS AND HEALTH, and on the Interdependence and Prevention of Diseases and the Diminution of their Fatality. Seventh edition, 8vo, 10s. 6d.

II.

AFFECTIONS OF THE HEART AND IN ITS NEIGHBOURHOOD. Cases, Aphorisms, and Commentaries. Illustrated by the heliotype process. 8vo, 6s 6d.

JOHN EAGLE.

Member of the Pharmaceutical Society.

A NOTE-BOOK OF SOLUBILITIES. Arranged chiefly for the use of Prescribers and Dispensers. 12mo, 2s 6d.

JOHN ERIC ERICHSEN.

Holme Professor of Clinical Surgery in University College; Senior Surgeon to University College Hospital, &c.

MODERN SURGERY; ITS PROGRESS AND TENDENCIES. Being the Introductory Address delivered at University College at the opening of the Session, 1873-74. Demy 8vo, 1s.

DR. FERBER.

MODEL DIAGRAM OF THE ORGANS IN THE THORAX AND UPPER PART OF THE ABDOMEN. With Letter-press Description. In 4to, coloured, 5s.

AUSTIN FLINT, JR., M.D.

Professor of Physiology and Physiological Anatomy in the Bellevue Medical College, New York; attending Physician to the Bellevue Hospital, &c.

I.

A TEXT-BOOK OF HUMAN PHYSIOLOGY; Designed for the Use of Practitioners and Students of Medicine. Illustrated by plates, and 313 wood engravings, large 8vo, 28s.

II.

THE PHYSIOLOGY OF MAN; Designed to Represent the Existing State of Physiological Science, as applied to the Functions of the Human Body. 5 vols., large 8vo. Vol. I.—The Blood; Circulation; Respiration. 18s. Vol. II.—Alimentation; Digestion; Absorption; Lymph and Chyle. 18s. Vol. III.—Secretion; Excretion; Ductless Glands; Nutrition; Animal Heat; Movements; Voice and Speech. 18s. Vol. IV.—The Nervous System. 18s. Vol. V.—Special Senses; Generation. 18s.

J. MILNER FOTHERGILL, M.D.

Member of the Royal College of Physicians of London; Physician to the City of London Hospital for Diseases of the Chest, Victoria Park, &c.

I.

THE HEART AND ITS DISEASES, WITH THEIR TREATMENT; INCLUDING THE GOUTY HEART. Second Edition, entirely re-written, copiously illustrated with woodcuts and lithographic plates. 8vo, 16s.

II.

INDIGESTION, BILIOUSNESS, AND GOUT IN ITS PROTEAN ASPECTS.

Part I.—INDIGESTION AND BILIOUSNESS.
Post 8vo, 7s 6d.

Part II.—GOUT IN ITS PROTEAN ASPECTS.
Post 8vo, 7s. 6d.

III.

HEART STARVATION. (Reprinted from the Edinburgh Medical Journal) 8vo, 1s.

ERNEST FRANCIS, F.C.S.

Demonstrator of Practical Chemistry, Charing Cross Hospital.

PRACTICAL EXAMPLES IN QUANTITATIVE ANALYSIS, forming a Concise Guide to the Analysis of Water, &c. Illustrated, fcap. 8vo, 2s. 6d.

HENEAGE GIBBES, M.D.

Lecturer on Physiology and Histology in the Medical School of Westminster Hospital; late Curator of the Anatomical Museum at King's College.

PRACTICAL HISTOLOGY AND PATHOLOGY. Second Edition, revised and enlarged. Crown 8vo, 5s.

C. A. GORDON, M.D., C.B.

Deputy Inspector General of Hospitals, Army Medical Department.

REMARKS ON ARMY SURGEONS AND THEIR WORKS. Demy 8vo, 5s.

SAMUEL D. GROSS, M.D., LL.D., D.C.L. OXON.

Professor of Surgery in the Jefferson Medical College of Philadelphia.

A PRACTICAL TREATISE ON THE DISEASES, INJURIES, AND MALFORMATIONS OF THE URINARY BLADDER, THE PROSTATE GLAND, AND THE URETHRA. Third Edition, revised and edited by S. W. Gross, A.M., M.D., Surgeon to the Philadelphia Hospital. Illustrated by 170 engravings, 8vo, 18s.

SAMUEL W. GROSS, A.M., M.D.

Surgeon to, and Lecturer on Clinical Surgery in, the Jefferson Medical College Hospital, and the Philadelphia Hospital, &c.

A PRACTICAL TREATISE ON TUMOURS OF THE MAMMARY GLAND; embracing their Histology, Pathology and Treatment. With Illustrations, 8vo, 10s 6d.

WILLIAM A. HAMMOND, M.D.

Professor of Mental and Nervous Diseases in the Medical Department of the University of the City of New York.

I.

A TREATISE ON THE DISEASES OF THE NERVOUS SYSTEM. Seventh Edition, with 112 Illustrations, large 8vo, 25s.

II.

A TREATISE ON INSANITY. With Illustrations, large 8vo, 25s.

III.

SPIRITUALISM AND ALLIED CAUSES AND CONDITIONS OF NERVOUS DERANGEMENT. With Illustrations, post 8vo, 8s. 6d.

ALEXANDER HARVEY, M.D.

Emeritus Professor of Materia Medica in the University of Aberdeen, &c.

AND

ALEXANDER DYCE DAVIDSON, M.D.

Professor of Materia Medica in the University of Aberdeen.

SYLLABUS OF MATERIA MEDICA FOR THE USE OF TEACHERS AND STUDENTS. Based on a selection or definition of subjects in teaching and examining; and also on an estimate of the relative values of articles and preparations in the British Pharmacopœia with doses affixed. Sixth Edition, 16mo, 1s 6d.

GRAILY HEWITT, M.D.

Professor of Midwifery and Diseases of Women in University College, Obstetrical Physician to University College Hospital, &c.

OUTLINES OF PICTORIAL DIAGNOSIS OF DISEASES OF WOMEN. Folio, 6s.

BERKELEY HILL, M.B. LOND., F.R.C.S.

Professor of Clinical Surgery in University College; Surgeon to University College Hospital, and to the Lock Hospital.

THE ESSENTIALS OF BANDAGING. For Managing Fractures and Dislocations; for administering Ether and Chloroform; and for using other Surgical Apparatus. Fifth Edition, revised and much enlarged, with Illustrations, fcap. 8vo, 5s.
[*Just Published.*

BERKELEY HILL, M.B. LOND., F.R.C.S.

Professor of Clinical Surgery in University College; Surgeon to University College Hospital, and to the Lock Hospital.

AND

ARTHUR COOPER, L.R.C.P., M.R.C.S.

Late House Surgeon to the Lock Hospital, &c.

I.

SYPHILIS AND LOCAL CONTAGIOUS DISORDERS. Second Edition, entirely re-written, royal 8vo, 18s.

II.

THE STUDENT'S MANUAL OF VENEREAL DISEASES. Being a Concise Description of those Affections and of their Treatment. Third Edition, post 8vo, 2s. 6d.

SIR W. JENNER, BART., M.D.

Physician in Ordinary to H.M. the Queen, and to H.R.H. the Prince of Wales.

THE PRACTICAL MEDICINE OF TO-DAY: Two Addresses delivered before the British Medical Association, and the Epidemiological Society. Small 8vo, 1s. 6d.

C. M. JESSOP, M.R.C.P.

Associate of King's College, London; Brigade Surgeon H.M.'s British Forces.

ASIATIC CHOLERA, being a Report on an Outbreak of Epidemic Cholera in 1876 at a Camp near Murree in India. With map, demy 8vo, 2s. 6d.

NORMAN W. KINGSLEY, M.D.S., D.D.S.

President of the Board of Censors of the State of New York; Member of the American Academy of Dental Science, &c.

A TREATISE ON ORAL DEFORMITIES AS A BRANCH OF MECHANICAL SURGERY. With over 350 Illustrations, 8vo, 16s.

E. A. KIRBY, M.D., M.R.C.S.

Late Physician to the City Dispensary.

I.

A PHARMACOPŒIA OF SELECTED REMEDIES WITH THERAPEUTIC ANNOTATIONS, Notes on Alimentation in Disease, Air, Massage, Electricity and other Supplementary Remedial Agents, and a Clinical Index; arranged as a Handbook for Prescribers. Sixth Edition, enlarged and revised, demy 4to, 7s.

II.

ON THE VALUE OF PHOSPHORUS AS A REMEDY FOR LOSS OF NERVE POWER. Fifth Edition, 8vo, 2s 6d.

J. WICKHAM LEGG, F.R.C.P.

Assistant Physician to Saint Bartholomew's Hospital, and Lecturer on Pathological Anatomy in the Medical School.

I.

ON THE BILE, JAUNDICE, AND BILIOUS DISEASES. With Illustrations in chromo-lithography, 719 pages, roy. 8vo, 25s.

II.

A GUIDE TO THE EXAMINATION OF THE URINE; intended chiefly for Clinical Clerks and Students. Fifth Edition, revised and enlarged, with additional Illustrations, fcap. 8vo, 2s 6d.

III.

A TREATISE ON HÆMOPHILIA, SOMETIMES CALLED THE HEREDITARY HÆMORRHAGIC DIATHESIS. Fcap. 4to, 7s 6d.

DR. GEORGE LEWIN.

Professor at the Fr. Wilh. University, and Surgeon-in-Chief of the Syphilitic Wards and Skin Disease Wards of the Charité Hospital, Berlin.

THE TREATMENT OF SYPHILIS WITH SUBCUTANEOUS SUBLIMATE INJECTIONS. Translated by DR. CARL PRŒGLE, and DR. E. H. GALE, late Surgeon United States Army. Small 8vo, 7s.

J. S. LOMBARD, M.D.

Formerly Assistant Professor of Physiology in Harvard College.

I.

EXPERIMENTAL RESEARCHES ON THE REGIONAL TEMPERATURE OF THE HEAD, under Conditions of Rest, Intellectual Activity and Emotion. With Illustrations, 8vo, 8s.

II.

ON THE NORMAL TEMPERATURE OF THE HEAD. 8vo, 5s.

WILLIAM THOMPSON LUSK, A.M., M.D.

Professor of Obstetrics and Diseases of Women in the Bellevue Hospital Medical College, &c.

THE SCIENCE AND ART OF MIDWIFERY. With numerous Illustrations, 8vo, 18s.

DR. V. MAGNAN.

Physician to St. Anne Asylum, Paris; Laureate of the Institute.

ON ALCOHOLISM, THE VARIOUS FORMS OF ALCOHOLIC DELIRIUM AND THEIR TREATMENT. Translated by W. S. GREENFIELD, M.D., M.R.C.P. 8vo, 7s. 6d.

A. COWLEY MALLEY, B.A., M.B., B.CH., T.C.D.

MICRO-PHOTOGRAPHY; including a description of the Wet Collodion and Gelatino-Bromide Processes, together with the best methods of mounting and preparing Microscopic Objects for Micro-Photography. With Illustrations and Photograph, crown 8vo, 5s. [*Just Published.*

PROFESSOR MARTIN.

MARTIN'S ATLAS OF OBSTETRICS AND GYNÆCOLOGY. Edited by A. MARTIN, Docent in the University of Berlin. Translated and edited with additions by FANCOURT BARNES, M.D. M.R.C.P., Physician to the Chelsea Hospital; Assistant Obstetric Physician to the Great Northern Hospital; and to the Royal Maternity Charity of London, &c. Medium 4to, Morocco half bound, 31s. 6d. net.

J. F. MEIGS, M.D.

Consulting Physician to the Children's Hospital, Philadelphia.

AND

W. PEPPER, M.D.

Lecturer on Clinical Medicine in the University of Pennsylvania.

A PRACTICAL TREATISE ON THE DISEASES OF CHILDREN. Seventh Edition, revised and enlarged, roy. 8vo, 28s.

PATRICK MANSON, M.D., C.M.

Amoy, China.

THE FILARIA SANGUINIS HOMINIS AND CERTAIN NEW FORMS OF PARASITIC DISEASES IN INDIA, CHINA, AND WARM COUNTRIES. Illustrated with Plates, Woodcuts, and Charts. Demy 8vo, 10s. 6d.

WILLIAM MARTINDALE, F.C.S.

Late Examiner of the Pharmaceutical Society, and Teacher of Pharmacy and Demonstrator of Materia Medica at University College.

AND

W. WYNN WESTCOTT, M.B. LOND.

Deputy Coroner for Central Middlesex.

THE EXTRA PHARMACOPŒIA of Unofficial Drugs and Chemical Pharmaceutical Preparations, with References to their use abstracted from the Medical Journals. Second edition, revised. Limp leather, med. 24mo. [*Nearly Ready.*

WM. JULIUS MICKLE, M.D., M.R.C.P.

Member of the Medico-Psychological Association of Great Britain and Ireland; Member of the Clinical Society, London; Medical Superintendent, Grove Hall Asylum, London.

GENERAL PARALYSIS OF THE INSANE. 8vo, 10s.

E. A. MORSHEAD, M.R.C.S., L.R.C.P.

Assistant to the Professor of Medicine in University College, London.

TABLES OF THE PHYSIOLOGICAL ACTION OF DRUGS. Fcap. 8vo, 1s.

A. STANFORD MORTON, M.B., F.R.C.S. ED.

Senior Assistant Surgeon, Royal South London Ophthalmic Hospital.

REFRACTION OF THE EYE: Its Diagnosis, and the Correction of its Errors, with Chapter on Keratoscopy. Second Edition. Small 8vo, 2s. 6d.

WILLIAM MURRELL, M.D., M.R.C.P., M.R.C.S.

Lecturer on Materia Medica and Therapeutics at Westminster Hospital; Senior Assistant Physician, Royal Hospital for Diseases of the Chest.

I.

WHAT TO DO IN CASES OF POISONING. Third Edition, revised and enlarged, med. 32mo, 2s. 6d. [*Now ready.*

II.

NITRO-GLYCERINE AS A REMEDY FOR ANGINA PECTORIS. Crown 8vo, 3s. 6d.

WILLIAM NEWMAN, M.D., F.R.C.S.

Surgeon to the Stamford Infirmary.

SURGICAL CASES: Mainly from the Wards of the Stamford, Rutland, and General Infirmary. 8vo, paper boards, 4s. 6d.

DR. FELIX VON NIEMEYER.

Late Professor of Pathology and Therapeutics; Director of the Medical Clinic of the University of Tübingen.

A TEXT-BOOK OF PRACTICAL MEDICINE, WITH PARTICULAR REFERENCE TO PHYSIOLOGY AND PATHOLOGICAL ANATOMY. Translated from the Eighth German Edition, by special permission of the Author, by GEORGE H. HUMPHREY, M.D., and CHARLES E. HACKLEY, M.D. Revised Edition, 2 vols., large 8vo, 36s.

C. F. OLDHAM, M.R.C.S., L.R.C.P.

Surgeon H.M. Indian Forces; late in Medical charge of the Dalhousie Sanitarium.

WHAT IS MALARIA? AND WHY IS IT MOST INTENSE IN HOT CLIMATES? An explanation of the Nature and Cause of the so-called Marsh Poison, with the Principles to be observed for the Preservation of Health in Tropical Climates and Malarious Districts. Demy 8vo, 7s. 6d.

G. OLIVER, M.D., M.R.C.P.

I.

THE HARROGATE WATERS: Data Chemical and Therapeutical, with notes on the Climate of Harrogate. Addressed to the Medical Profession. Crown 8vo, with Map of the Wells, 3s. 6d.

II.

ON BEDSIDE URINE TESTING: Qualitative Albumen and Sugar Fcap. 8vo, 1s. 6d.

JOHN S. PARRY, M.D.

Obstetrician to the Philadelphia Hospital, Vice-President of the Obstetrical and Pathological Societies of Philadelphia, &c.

EXTRA-UTERINE PREGNANCY; Its Causes, Species, Pathological Anatomy, Clinical History, Diagnosis, Prognosis and Treatment. 8vo, 8s.

E. RANDOLPH PEASLEE, M.D., LL.D.

Late Professor of Gynæcology in the Medical Department of Dartmouth College; President of the New York Academy of Medicine, &c., &c.

OVARIAN TUMOURS: Their Pathology, Diagnosis, and Treatment, especially by Ovariotomy. Illustrations, roy. 8vo, 16s.

G. V. POORE, M.D., F.R.C.P.

Professor of Medical Jurisprudence, University College; Assistant Physician and Physician in charge of the Throat Department of University College Hospital.

LECTURES ON THE PHYSICAL EXAMINATION OF THE MOUTH AND THROAT. With an appendix of Cases. 8vo, 3s. 6d.

R. DOUGLAS POWELL, M.D., F.R.C.P.

Physician to the Hospital for Consumption and Diseases of the Chest at Brompton, Physician to the Middlesex Hospital.

ON CONSUMPTION AND ON CERTAIN DISEASES OF THE LUNGS AND PLEURA. Being a Second Edition revised and extended of "The Varieties of Pulmonary Consumption." Illustrated by woodcuts and a coloured plate, 8vo, 9s.

AMBROSE L. RANNEY, A.M., M.D.

Adjunct Professor of Anatomy in the University of New York, etc.

THE APPLIED ANATOMY OF THE NERVOUS SYSTEM, being a study of this portion of the Human Body from a stand-point of its general interest and practical utility, designed for use as a Text-book and a work of Reference. With 179 Illustrations, 8vo, 20s.

RALPH RICHARDSON, M.A., M.D.

Fellow of the College of Physicians Edinburgh.

ON THE NATURE OF LIFE: An Introductory Chapter to Pathology. Second Edition, revised and enlarged, Fcap. 4to 10s. 6d.

SYDNEY RINGER, M.D.,

Professor of the Principles and Practice of Medicine in University College; Physician to and Professor of Clinical Medicine in, University College Hospital.

I.

A HANDBOOK OF THERAPEUTICS. Tenth Edition, revised, 8vo, 15s. [*Now Ready.*

II.

ON THE TEMPERATURE OF THE BODY AS A MEANS OF DIAGNOSIS AND PROGNOSIS IN PHTHISIS. Second Edition, small 8vo, 2s. 6d.

FREDERICK T. ROBERTS, M.D., B.SC., F.R.C.P.

Examiner in Medicine at the Royal College of Surgeons; Professor of Therapeutics in University College; Physician to University College Hospital; Physician to the Brompton Consumption Hospital, &c.

A HANDBOOK OF THE THEORY AND PRACTICE OF MEDICINE. Fifth Edition, with Illustrations, in one volume, large 8vo, 21s.

D. B. ST. JOHN ROOSA, M.A., M.D.

Professor of Diseases of the Eye and Ear in the University of the City of New York; Surgeon to the Manhattan Eye and Ear Hospital; Consulting Surgeon to the Brooklyn Eye and Ear Hospital, &c., &c.

A PRACTICAL TREATISE ON THE DISEASES OF THE EAR, including the Anatomy of the Organ. Fourth Edition, Illustrated by wood engravings and chromo-lithographs, large 8vo, 22s.

ALDER SMITH, M.B. LOND., F.R.C.S.

Resident Medical Officer, Christ's Hospital, London.

RINGWORM: ITS DIAGNOSIS AND TREATMENT. Second Edition, rewritten and enlarged. With additional Illustrations, fcap. 8vo, 4s. 6d.

J. LEWIS SMITH, M.D.

Physician to the New York Infants' Hospital; Clinical Lecturer on Diseases of Children in Bellevue Hospital Medical College.

A TREATISE ON THE DISEASES OF INFANCY AND CHILDHOOD. Fifth Edition, with Illustrations, large 8vo, 21s.

JAMES STARTIN, M.B., M.R.C.S.

Surgeon and Joint Lecturer to St. John's Hospital for Diseases of the Skin.

LECTURES ON THE PARASITIC DISEASES OF THE SKIN. Vegetoid and Animal. With Illustrations, crown 8vo, 3s. 6d.

LEWIS A. STIMSON, B.A., M.D.

Surgeon to the Presbyterian Hospital; Professor of Pathological Anatomy in the Medical Faculty of the University of the City of New York.

A MANUAL OF OPERATIVE SURGERY. With three hundred and thirty-two Illustrations. Post 8vo, 10s. 6d.

HUGH OWEN THOMAS, M.R.C.S.

DISEASES OF THE HIP, KNEE, AND ANKLE JOINTS, with their Deformities, treated by a new and efficient method. With an Introduction by RUSHTON PARKER, F.R.C.S., Lecturer on Surgery at the School of Medicine, Liverpool. Third Edition, 8vo, 25s.

HUGH OWEN THOMAS, M.R.C.S.

AND

RUSHTON PARKER, F.R.C.S.

Lecturer on Surgery at the Liverpool Royal Infirmary.

THE TREATMENT OF INTESTINAL DISEASES AND OBSTRUCTIONS. Third Edition, with Lithographic Plates. 8vo, 10s.

J. C. THOROWGOOD, M.D.

Assistant Physician to the City of London Hospital for Diseases of the Chest.

THE CLIMATIC TREATMENT OF CONSUMPTION AND CHRONIC LUNG DISEASES. Third Edition, post 8vo, 3s. 6d.

J. ASHBURTON THOMPSON, M.R.C.S.

FREE PHOSPHORUS IN MEDICINE WITH SPECIAL REFERENCE TO ITS USE IN NEURALGIA. A contribution to Materia Medica and Therapeutics. An account of the History, Pharmaceutical Preparations, Dose, Internal Administration, and Therapeutic uses of Phosphorus; with a Complete Bibliography of this subject, referring to nearly 200 works upon it. Demy 8vo, 7s. 6d.

LAURENCE TURNBULL, M.D., PH.G.

Aural Surgeon to Jefferson Medical College Hospital, &c., &c.

ARTIFICIAL ANÆSTHESIA: A Manual of Anæsthetic Agents, and their Employment in the Treatment of Disease. Second Edition, with Illustrations, crown 8vo, 6s.

W. H. VAN BUREN, M.D. LL.D.

Professor of Surgery in the Bellevue Hospital Medical College.

DISEASES OF THE RECTUM: and the Surgery of the Lower Bowel. Second Edition, with Illustrations, 8vo, 14s.

RUDOLPH VIRCHOW, M.D.

Professor in the University, and Member of the Academy of Sciences of Berlin, &c.

INFECTION-DISEASES IN THE ARMY, chiefly Wound Fever, Typhoid, Dysentery, and Diphtheria. Translated from the German by JOHN JAMES, M.B., F.R.C.S. Fcap. 8vo, 1s. 6d.

ALFRED VOGEL, M.D.

Professor of Clinical Medicine in the University of Dorpat, Russia.

A PRACTICAL TREATISE ON THE DISEASES OF CHILDREN. Translated and Edited by H. RAPHAEL, M.D. From the Fourth German Edition, Illustrated by six lithographic plates, part coloured, large 8vo, 18s.

A. DE WATTEVILLE, M.A., B.SC., M.R.C.S.

Assistant Physician to the Hospital for Epilepsy and Paralysis, late Electro-Therapeutical Assistant to University College Hospital.

A PRACTICAL INTRODUCTION TO MEDICAL ELECTRICITY. Second Edition. [*In the Press.*

W. SPENCER WATSON, F.R.C.S., M.B.

Surgeon to the Great Northern Hospital; Surgeon to the Royal South London Ophthalmic Hospital.

I.

EYEBALL TENSION: its Effects on the Sight and its Treatment. With woodcuts, post 8vo, 2s. 6d.

II.

DISEASES OF THE NOSE AND ITS ACCESSORY CAVITIES. Profusely Illustrated. Demy 8vo, 18s.

III.

ON ABSCESS AND TUMOURS OF THE ORBIT. post 8vo, 2s. 6d.

FRANCIS WELCH, F.R.C.S.

Surgeon-Major, A.M.D.

ENTERIC FEVER: its Prevalence and Modications; Etiology; Pathology and Treatment; as illustrated by Army Data at Home and Abroad. Demy 8vo, 5s. 6d.

DR. F. WINCKEL.

Formerly Professor and Director of the Gynæcological Clinic at the University of Rostock.

THE PATHOLOGY AND TREATMENT OF CHILDBED: A Treatise for Physicians and Students. Translated from the Second German Edition, with many additional notes by the Author, by J. R. Chadwick, M.D. 8vo 14s.

EDWARD WOAKES, M.D.

Senior Aural Surgeon and Lecturer on Aural Surgery at the London Hospital; Senior Surgeon to the Hospital for Diseases of the Throat.

I.

ON DEAFNESS, GIDDINESS, AND NOISES IN THE HEAD. Third Edition, with Illustrations. [*In the Press.*

II.

CATARRH, AND DISEASES OF THE NOSE CAUSING DEAFNESS. With illustrations. [*In the Press.*

E. T. WILSON, B.M. OXON., F.R.C.P. LOND.

Physician to the Cheltenham General Hospital and Dispensary.

DISINFECTANTS AND HOW TO USE THEM. In Packets of one doz. price 1s.

CLINICAL CHARTS FOR TEMPERATURE OBSERVATIONS, ETC.

Arranged by W. Rigden, M.R.C.S. Price 7s. per 100, or 1s. per doz.

Each Chart is arranged for four weeks, and is ruled at the back for making notes of cases; they are convenient in size, and are suitable both for hospital and private practice.

⁂ Mr. Lewis has transactions with the leading publishing firms in America for the sale of his publications in that country. Arrangements are made in the interests of Authors either for sending a number of copies of their works to the United States, or having them reprinted there, as may be most advantageous.

Mr. Lewis's publications can be procured of any bookseller in any part of the world.

List of works issued for the New Sydenham Society on application.

London: Printed by H. K. Lewis, Gower Street, W.C.